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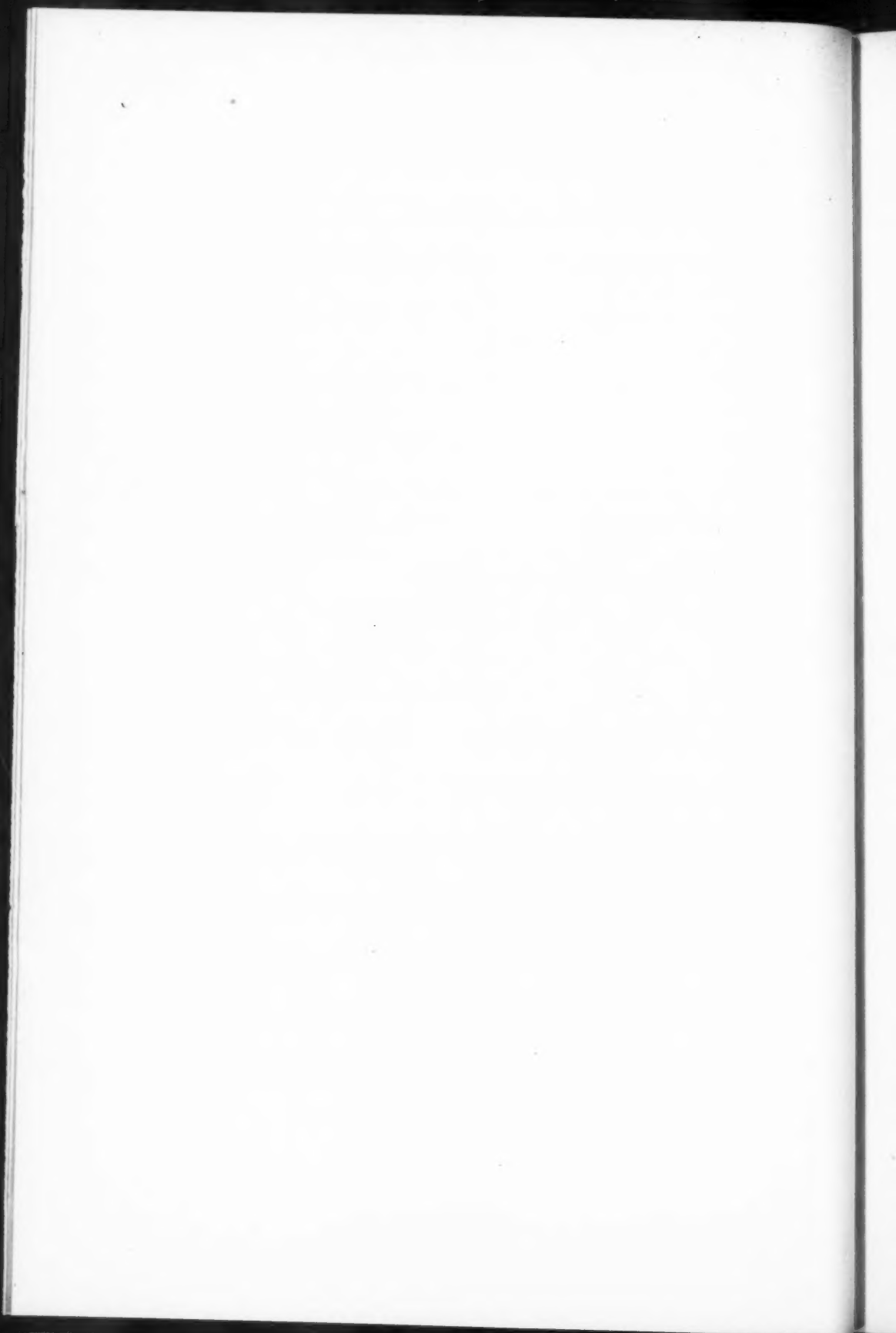
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ARE THE EFFECTS OF SIMPLE OVERSTRAIN MONOTROPIC?

BY HENRY M. HOWE.

SUMMARY.

This paper points out the need of further study to show whether the strengthening effect of prior overstrain, by cold rolling, cool rolling, wire-drawing, etc., is effective or fictitious as regards the endurance of repetitive stresses, whether of constant or of reversing sign.

It considers further how the effects of overstrain are related to the direction of the deformation which the overstrain causes, asking for instance whether those effects are monotropic, pan-tropic, or isotropic. It may be recapitulated as follows.

1. Simple overstrain must be distinguished from that which is preceded by stress reversals. (Section 2.)

2. Simple overstrain seems to have less strengthening effect if preceded by stress reversals than if not so preceded. (Section 3.)

3. The evidence which seems to show that the effects of reversing overstrain differ from those of simple overstrain in being far less fully removed by so-called annealing, is really inconclusive, because the temperatures used, rising only to 320° C., are not annealing but stiffening towards steel which has undergone simple overstrain. (Section 5.)

4. Reversing overstrain, instead of adding to the strengthening caused by prior overstrain, appears to efface it. (Section 6.)

5. Both reversing and non-reversing repetitive overstrain lack the hardening and probably the lightening effect of simple overstrain. (Section 7.)

6. The propriety of basing specifications for services implying reversing stresses on test pieces annealed at 600° C. is discussed. (Section 8.)

7. A like discussion is given concerning services implying repeated wide stress ranges without change of sign. (Section 9.)

8. The phenomena which suggest that the effects of initial overstrain persist are considered. (Section 11.)

9. Bairstow's evidence supporting Bauschinger's hypothesis is interpreted not as strict confirmation but as contributory support. (Section 12.)

10. Familiar examples are cited indicating that the strengthening caused by overstrain is not monotropic. (Section 13.)

11. Evidence is offered that simple overstrain increases the Brinell hardness isotropically in steel. (Section 14.)

12. Evidence of the isotropy of simple overstrain in other metals is cited. (Section 14.)

13. Evidence apparently interpreted as proving monotropy of simple overstrain is shown to be inconclusive. (Section 15.)

ARE THE EFFECTS OF SIMPLE OVERSTRAIN MONOTROPIC?

BY HENRY M HOWE.

1. *Introductory.*—This paper is offered as a contribution from the Metallurgical Laboratories of Columbia University. The immediate purpose is to present some evidence tending to show that the strengthening caused by a simple overstrain, such as occurs in tension testing, is not confined to resistance to later stress in the same direction, as seems to be held currently, but is a strengthening in all directions. The presentation of this evidence I defer to Section 14, devoting the earlier sections chiefly to showing that it is needed and to pointing out regrettable gaps in our present knowledge, with the hope of stimulating others to fill them.

Going beyond the two general conceptions that the effects of overstrain may be either (1) *isotropic*, identical in degree and sign in every direction, or (2) not isotropic but *anisotropic*, we may distinguish for discussion two cases among the many conceivable forms of anisotropy, (3) one in which the overstrain strengthens the metal against later stress in the same direction but weakens it against such stress in the opposite direction, and (4) one in which it strengthens the metal in all directions, though to a degree which varies with the direction of that later stress, being greatest for stresses which are in the same direction as the overstrain itself. Items (3) and (4) are special forms of *monotropy* and *panropy* respectively, names which, as a temporary expedient, I shall use in this paper for brevity and precision, cautioning the reader of this artificial and temporary restriction of the meaning of these two words.

2. *Simple and Reversing Overstrain.*—Though the effects of the overstrain which occurs in connection with repeated reversals of stress may be clearly anisotropic, there is much to indicate that some at least of the effects of a single overstrain are isotropic.

In what follows I discriminate sharply between overstrain

which is, and that which is not, preceded by stress reversals in excess of the elastic range, such for instance as occur in the endurance test, or in an unduly loaded piston rod or crank shaft.

The anisotropy of overstrain in connection with reversals of stress is illustrated by the fact that, after the so-called "natural elastic limits" of Bauschinger, equal in compression and tension, have been established by alternations of equal tensile and compressive hyper-elastic stresses, the proportionality limit in tension can be raised and that in compression simultaneously lowered by changing the range of these alternations from equal tension and compression to greater tension and less compression, indeed to the last tensile and compressive stresses respectively reached in those alternations.¹

Again, to paraphrase the language of a recent writer, if, after undergoing many reversals of stress, "the proportionality limit of a piece of steel is raised in tension by previous overstraining in tension, the proportionality limit of the piece in compression is at the same time lowered, so that while the metal has been 'hardened' as against further tension, it has actually been 'softened' as against compression."²

From this unquestionable anisotropy thus set up by stress reversals, it seems to have been inferred currently that similar anisotropy is true not only of unreversed overstrain in general but even of single applications of overstrain. Indeed one of the most learned and brilliant of the investigators in this field has lately said that, in overstrained steel, "the hardness is in reality limited to the particular direction in which the strain has been applied." Without here asking how accurately this is true of overstrain which is preceded by stress reversals, let us ask in Section 13 whether it is true of simple overstrain which is not so preceded.

3. Influence of Stress Reversals on the Effects of Simple Overstrain.—Such stress reversals seem to affect profoundly

¹ For an interesting illustration of this see Stanton and Bairstow "On the Resistance of Iron and Steel to Reversals of Direct Stress," *Collected Researches, National Physical Laboratory*, Vol. III, p. 31 (1907), and *Proceedings, Inst. Civ. Engrs.*, Session 1905-6, p. 21 of reprint.

² For a very explicit confirmation of this law see Bairstow, *Collected Researches, National Physical Laboratory*, Vol. VI, p. 187 (1910); also *Philosophical Trans. Roy. Soc., A*, Vol. 210, p. 55 (1910).

the influence of overstrain. Thus, though most of the effects of overstrain uncomplicated by prior stress reversals are explained satisfactorily by Beilby's theory of amorphous metal, these stress reversals seem to introduce some very complicating phenomena which that theory in its present form seems wholly incompetent to meet, so that it now needs an important amendment or extension.

An illustration of this complicating influence of prior stress reversals is that, whereas simple overstrain raises the yield point very closely to the overstraining stress, and if followed by sufficient rest or by gentle heating raises the proportionality limit beyond that stress, yet when such overstrain is preceded by stress reversals, then even if followed by a competent heating, instead of raising the proportionality limit beyond the overstraining stress it may fail to raise it even to that stress.¹

4. *Three Apparent Differences between the Effects of Simple and Reversing Overstrain.*—There has been reason to suspect that the effects of reversing overstrain differ materially, at least in degree, from those of simple overstrain, and at least in three respects: (1) that they are less fully removed by a so-called annealing; (2) that they are not cumulative and additive towards the effects of prior overstrain; and (3) that they cause a smaller increase, if any, of the Brinell hardness.

For the second of these differences I find no sufficient explanation, but for the first and third I suggest explanations, the validity of which remains to be tested.

5. *Difference as Regards Annealing(?)*.—Annealing(?) seems to remove the effects of reversing overstrain far less fully than those of simple overstrain. To anneal in wire-drawing, cold-rolling, or other form of simple overstrain, removes practically completely the effects of the overstrain which has thus far occurred, and in this way systematic repetitions of annealing

¹See Bairstow, *Collected Researches, National Physical Laboratory, Vol. VI, p. 187* (1910). Steel, which initially had a proportionality limit and yield point of 24.9 tons per sq. in., was subjected to reversals of stress. After this its proportionality limit persistently refused to be raised beyond 19.92 tons per sq. in. by overstrains of 26.56 tons per sq. in., each followed by heating to 100° C. Yet Muir had shown that, in the absence of such prior stress reversals, steel overstrained well into its yield point of about 60,000 lb. per sq. in. and then heated thus at 100° C. for 15 minutes had then a proportionality limit of about 92,000 lb. per sq. in., overstrain followed by heating to 100° having raised the proportionality limit to about 30,000 lb. per sq. in. above the overstraining stress. (*Philosophical Trans. Roy. Soc., A, Vol. 193, 1899.*)

enable us to increase the reduction of area indefinitely. If annealing had a like effect on reversing overstrain, then systematic annealings would increase the endurance indefinitely. Of course, in order to be effective they should begin before cracks have actually started, and should be repeated. Ideally, each annealing might be so applied as to increase the endurance one fold, so that six annealings would increase it 600 per cent; and though such ideal conditions cannot be closely approached, yet the increase of endurance by repeated annealings ought to be very great. But among 17 of Howard's cases,¹ assembled by him in 1890, in which there were from 6 to 8 annealings, beginning early in the endurance test, the average increase of endurance was only 60 per cent, the maximum increase was 210 per cent, and in 4 of the 17 cases the repeated annealings were followed by a loss of endurance.

In one case five like specimens were tested for endurance, one without annealing, the others with six annealings each, during the progress of the endurance test. In all but one of these tests the annealings lessened the endurance slightly, and in that one the net gain by the six annealings was only 3 per cent, or within the limits of error.

It is true that he generally found much greater endurance when the test pieces were between 100 and 370° C. than when they were artificially cooled with a stream of water. In the case of seven "Gautier" steels the increase of the endurance caused by running at temperatures between 166 and 245° C. was very great, between 2½ and 18½ fold.²

Such hot running at first sight seems to be a continuous annealing. Unwin, too, found in several but not all cases a moderate increase of endurance on running his tests hot, up to 230° C. (449° F.).³ He seems inclined to refer the effect of hot running not so much to the temperature of the tests as to the cooling down at night, perhaps inferring, from the fact that the rate of cooling from above Ar1 has an important effect, that the rate of cooling from these low temperatures also has an important effect, an inference which is probably wrong. At

¹ J. E. Howard, "Tests of Metals," Watertown Arsenal, 1890, p. 1050.

² *Ibid.*, 1893, p. 488.

³ Unwin, "The Testing of Materials of Construction," 1910, p. 385.

these sub-transformation temperatures it is the length of exposure to given temperature that counts, not the rate of change of temperature.

But the whole of this evidence is inconclusive, because Howard's temperatures, rising only to 320°C ., may cause not annealing but stiffening. There is ample evidence to show that moderate reheating, certainly to above 300° , raises the elastic limit of overstrained steel; indeed industrial processes are based on this. Were the effects of repetitive overstrain identical with those of simple overstrain, then such heating would be expected rather to hasten the elevation of the elastic limit caused in the endurance test.

Evidently further experiments are needed to show the effect of a true annealing on the endurance.

6. *Difference as Regards Prior Overstrain*.—Though simple overstrain followed by rest raises the tensile strength, yield point, and proportionality limit, and though these effects are repetitive and cumulative, repeated tensile overstrains even when pushed to rupture raising these properties higher and higher, yet there is much evidence to show that the overstraining stress reversals of the endurance test, whether pushed to actual rupture or not, do not raise these properties to any comparable degree.¹

¹ Stanton and Bairstow found that reversals of stress from tension to compression caused a very great lessening of the tensile elastic limit. (Collected Researches, National Physical Laboratory, Vol. III, p. 31 (1907), and *Proceedings*, Inst. Civ. Engrs., Session 1905-6, p. 21 of reprint.) Howard found that such reversals from equal tension to equal compression in shafts rotating while bent, affected the tensile strength but slightly. There was a gain of tenacity in half his cases which I have examined, and a loss in the other half, the average loss being greater than the average gain. (J. E. Howard, "Tests of Metals," Watertown Arsenal, 1888, p. 781.)

When repetitions of tensile stress from zero to a maximum are substituted for these reversals of stress from tension to compression, the effect is not so different as might be expected. Bauschinger found that after repetitions to rupture of overstrain in direct tension, with the stress varying between zero and a maximum, the tensile strength was on the whole nearly the same as before. These repetitions in some cases doubled or even more than doubled the elastic limit, but on others they lessened it materially. (Unwin, "The Testing of Materials of Construction," 1910, p. 382.)

Here Howard's conditions give much greater weight to his results for obvious reasons. Test pieces which are undergoing the endurance test are strained equally from end to end if that test is either wholly tensile as in Bauschinger's case, or is alternately in direct axial tension and compression as in the tests of Stanton and Bairstow. Hence tension test pieces, cut from the fragments of test pieces broken in the endurance test, might well have become fatigued in that test nearly to the point of breaking. Therefore such test pieces might well

Indeed there is much evidence to show that reversing overstrain, instead of adding to any initial elevation of the elastic limit and tensile strength caused by prior overstrain, as simple overstrain does, for example, in wire-drawing and cold-rolling, has the opposite result of effacing the effects of such overstrain.¹ If this is true, then it follows that any rise of the tensile strength and elastic limit caused by prior overstrain is fictitious as regards the endurance of stress reversals, and that the effective fatigue strength is that which existed before such prior overstrain, and hence that to which the metal would return on removing that overstrain by annealing at 600° C. This will be considered further in Section 10.

This hypothesis, that reversing overstrain (that is, the repetition and reversal of stresses in excess of the natural elastic

prove in the subsequent tension test weaker than the initial metal, and this would mean only that repetitions pushed nearly to rupture have a weakening effect.

But in Howard's cases the endurance test was by rotating a shaft while bent. Under these conditions only that part of the shaft in the middle of its length would be fatigued nearly to rupture. Tension test pieces from other parts of the fragments of such an endurance test piece would evidently not have been fatigued nearly to rupture, and indeed might not have been strained beyond their elastic limit at all. Loss of strength in this case means that reversing stresses, even if they fall very far short of being pushed to rupture, nevertheless have a weakening effect.

It is true that Howard sometimes found a marked increase of strength caused by his stress reversals, so that the case is not closed. But the loss of strength caused in so many cases of stress reversals which have not approached rupture is sufficient for our present purpose of establishing a marked difference between the effects of simple and those of reversing overstrain.

¹ The Stanton and Bairstow results cited in a previous footnote are evidence to this effect. The loss of elastic limit noted by Bauschinger, and cited in the same footnote, may be interpreted as indicating that the elastic limit of the initial material had been raised by cold-rolling or otherwise, and that this elevation was effaced by the repetitions of stress, though these were not reversed, being from zero to a maximum. The same is true of Bairstow's depression of the elastic limit by reversing stresses, cited in the footnote to Section 3.

So with Howard's results by reversals of stress in the endurance test on bars rotating while bent, and thus passing from a tensile to an equal compressive stress. He determined in this way the endurance of specimens of cold-rolled wrought iron, and, after he had thus broken them by fatigue he cut from their fragments tension test pieces which he next tested. Those from mid-length of the endurance test piece, where the stress probably exceeded the natural elastic limit, so that there was reversing overstrain, were now much weaker than those from the ends of the endurance piece where the local stresses were so slight that probably no reversing overstrain occurred. (J. E. Howard, "Tests of Metals," Watertown Arsenal, 1892, p. 524.) Here we may infer that the reversing overstrain mid-length of the endurance piece lessened or even effaced the great strengthening which had been caused by the initial cold-rolling.

Tests on seven "Gautier" steels pointed in the same direction, though less strongly. Tensile tests were made on pieces cut from the fragments of those broken in the endurance test. On the whole the tensile test pieces cut from mid-length of the endurance bar were weaker than those cut from the ends of those bars, the middle being weaker than the ends in 31 cases, on an average by 11,350 lb. per sq. in., and stronger than the ends in 27 cases, on an average by 4480 lb. per sq. in. (J. E. Howard, "Tests of Metals," Watertown Arsenal, 1895, p. 601.)

limits of the annealed material) effaces the strengthening effects of prior overstrain, if broadly true, would imply that cold-rolled shafting, hard-drawn wire, and like materials have no greater endurance than the same materials when annealed, a proposition which at first seems opposed to common observation or at least to firm belief. I am now seeking information on this point, but as it is not essential to the chief purpose of this paper I do not wait for it.

7. *Does either Reversing or Repetitive Overstrain Harden Steel as Simple Overstrain Does?*—Tensile rupture increases the Brinell hardness of low-carbon steel by about 40 Brinell numbers in Mesnager's results,¹ and by about 60 numbers in the results in Fig. 4. Wire-drawing has a much greater hardening effect,—between about 80 and about 145 Brinell numbers in Goerens' data.² But the data which I give in Table I indicate that the hardening effect of repetitions of stress cycles pushed to rupture is much less, whether those cycles are of constant or of reversing sign.

No difference in hardness could be detected in specimen A between the fatigue-broken and the unfatigued parts of the bar, even when the determinations were made on the very fracture itself without any grinding off. There was a slight but constant difference between the hardness of the longitudinal and transverse sections, perhaps due to uneffaced structural heterogeneity set up in rolling. The hardness, 332 to 340, indicates that the drawing at a blue heat does not completely remove the effects of the prior quenching, as indeed is well known. Brinell gave the hardness of a 1-per-cent carbon steel as 259 when annealed and 627 when hardened. This residual hardening due to quenching might lessen the increased hardness which the overstrain would induce, but it does not explain why the overstrain caused no increase at all.

Specimen B was slightly harder, and specimen C slightly softer, on the ground-down fracture than on the unstrained end, but the differences are much smaller than in the case of tensile rupture.

¹ Internat. Assoc. Test. Mats., Reunion Members Francais et Belges, March 23, 1907, p. 14.

² Iron and Steel Institute, Carnegie Scholarship Memoirs, Vol. 3, Fig. 10, p. 342 (1911).

TABLE I.—REVERSING OVERSTRAIN DOES NOT HARDEN STEEL AS SIMPLE OVERSTRAIN DOES.

Specimen.	Test No.	Part Represented.	Brinell Hardness Number.		Specific Gravity.		Carbon Content, per cent.
			Transverse Section.	Longitudinal Section.	Details.	Average.	
A	1	Fracture, ground smooth, end.	332	340	7.840, 7.837	7.839	1±
"	2	" " " center.	332	340			
"	3	Fracture, unground, end.	332	340	7.836, 7.844, 7.843	7.841	
"	4	" " center.	332	340			
"	5	Unstrained end.	332	340	7.841, 7.840, 7.841	7.841	
"		Difference, fracture minus unstrained. . .	0	0		-0.001	
B	6	Fracture.	143	126	7.856, 7.842, 7.850	7.853	0.25
"	7	Unstrained end.	126	124	7.877, 7.873	7.873	
"		Difference, fracture minus unstrained. . .	+17	+2		-0.025	
C	8	Fracture.	277	286	7.845, 7.838, 7.845	7.843	0.385
"	9	Unstrained end.	286	286	7.838, 7.841, 7.838	7.839	
"		Difference, fracture minus unstrained. . .	-9	0		+0.004	

NOTE TO TABLE I.—The Brinell hardness was determined both on longitudinal sections as near the fracture as practicable, centering the ball at about $\frac{1}{8}$ in. from the fracture, and also on transverse sections. These latter sections, for tests Nos. 1, 2, 6 and 8, were prepared by grinding down the fracture wet, removing as little as would suffice to give a smooth surface. In addition two determinations, Nos. 3 and 4, were made with specimen A on the very fracture, which was unusually smooth. This part of the fracture was cut off from the specimen before any grinding was done, so that there is no possibility of its having been heated or otherwise affected by the grinding.

The ball had a diameter of 9.75 mm., and the pressure was 6700 lb. (3000 kg.). The specific gravity determinations were made before determining hardness.

Specimen A was 2 in. wide, $\frac{1}{4}$ in. thick, and about 14 $\frac{1}{2}$ in. long. In the endurance test it lay on its flat side, supported at both ends, and was repeatedly bent down by the pressure applied at its mid-length. The fatigue test was made, and the specimen kindly given, by Mr. C. D. Young, Engineer of Tests of the Pennsylvania Railroad Co., Altoona, Pa. Tests Nos. 2 and 4 were made near the center of the 2 by $\frac{1}{4}$ -in. face, that is, transverse section, at the fracture; tests Nos. 1 and 2 at the ends of this same section. Specimens B and C were kindly given by Mr. Henry Souther, President of the Henry Souther Engineering Company. The stress cycle for specimen A was from zero to a tensile maximum, that for specimens B and C was from a tensile maximum to an approximately equal compressive maximum.

The composition and properties of these steels are given in the following table:

Specimen.	Composition, per cent.				Tensile Properties.				Endurance Test.		Condition.
	C	Mn	Ni	Cr	Ultimate Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	Fiber Stress, lb. per sq. in.	Repetitions before Failure.	
A	1	70 000	212 000	Water-quenched, spring-tempered (dark blue). Natural state, as received.
B	0.25	0.59	3.49	..	79 800	55 400	29.5	61.8	54 800	75 400	
C	0.385	0.40	1.18	1.17	109 200	84 600	18.5	57.4	54 800	1 495 000	Oil-quenched from 816° C., drawn at 566° C.

The preliminary determinations of density in Table I do not indicate that fatigue overstrain has so marked a lightening effect as simple overstrain is now well known to have, though in the case of specimen A this may perhaps represent the anomalous behavior of high-carbon steel found by Brunton.¹

Here are indications that the effects of repetitive overstrain either differ markedly from those of simple overstrain, or else are very closely confined to the immediate region of rupture.

This difference as regards hardening and lightening might, indeed, be foreseen. Fatigue concentrates the deformation into a relatively smaller number of slipping planes, and this restriction of the deformation would naturally cause a like restriction of the hardening and lightening to the immediate neighborhood of those planes, leaving the undeformed metal outside these immediate regions unhardened and unlightened. Simple overstrain, as in tensile rupture, wire-drawing, etc., causes far more widespread deformation.

Hence this evidence is not enough to establish any difference in kind between the effects of simple and those of reversing or repetitive overstrain. We have already seen that the evidence as to annealing also is insufficient. But there remains the difference as regards the effacement of prior overstrain, which thus far seems to be a real difference.

8. *Should Engineering Specifications for Unhardened Reverse-Stress Objects be Based on Annealing at 600° C.?*—If it is true that stress reversals efface the elevation of the elastic limit due to prior overstrain, for example, that caused by cool-rolling or forging, by quenching stresses, etc., then it seems to follow that engineering specifications for objects which in use are subjected to numerous stress reversals should ignore such elevation, and hence that this elevation should be effaced in the test pieces which testify to the fitness of the metal. This effacing, according to our present evidence, is effected fully by a simple heating to 600° C., which, if not unduly prolonged, ought not to cause any other important change in unhardened steel, such as de-sorbitizing that which has been heat-treated skillfully. Such a 600°-heating of course could not apply to steel which had been hard-

¹ *Journal, Iron and Steel Institute*, No. 2, Vol. 70, p. 154 (1906).

ened and partly drawn, such as spring steel, because it would complete the drawing, and thus substitute the annealed for the tempered state sought. Nor would it be needed for heat-treated axles and like objects, for any usual heat treatment itself would do that very effacing of prior overstrain which the 600°-heating aims to bring about.

9. *Should Structural Pieces not Subjected to Stress Reversals, but only to Great Stress Changes, be Dis-overstrained?*—Is the elevation of the proportionality limit by cool-rolling, forging, etc., fictitious as regards stresses which, though varying within wide limits, yet remain constant in sign? I interpret Bairstow's evidence as arguing that it is.

In order to understand this inference as set forth in Section 11, we should note certain fundamental facts. For a given piece of steel, we may recognize the "primitive elastic limit," that which is found in the tensile test, and the "natural elastic limits" of Bauschinger, the limits within which the steel remains elastic during indefinitely repeated stress cycles. These natural elastic limits, for any given piece of steel, can, by subjecting it to repeated stress cycles, be raised or lowered at will as sketched in Fig. 1, in which abscissas represent the lower limit of the stress cycle, and ordinates the width of the stress range within which the steel remains wholly elastic during cycles which start from a given lower limit. Here it is convenient to regard compressive stresses as simply minus tensile stresses. Clearly the upper limit of the elastic range corresponding to any given lower limit is found by drawing a line sloping down to the right at 45 deg. from the curve, at the point where it is cut by the ordinate.

To explain: if we arbitrarily adopt point *C*, of -12 tons per sq. in., as the lower limit of the stress cycle to which we expose our specimen, then in order that the steel shall remain wholly elastic, the curve teaches that the stress range must not exceed 25 tons per sq. in., and hence the upper limit of the cycle must not exceed $-12 + 25 = 13$ tons per sq. in., or the point *H* found by drawing *CH* at an angle of 45 deg. from *C*. If on the other hand, we arrange matters so that during the stress cycle the stress shall vary between a lower limit of zero and an upper limit *X*—that is, that a given tensile stress, *X*, shall be alter-

nately applied and fully released,—then the curve teaches that the elastic stress range is *D* or 21.5 tons per sq. in., and the maximum stress which can be applied without exceeding the elastic limit is $0+21.5=21.5$ tons per sq. in. If finally, our conditions are such that the stress, while always remaining positive, shall be released only till it has fallen to 15 tons per sq. in., or *G*, then the width of the elastic range, *G*, is 18 tons per sq. in., and hence the upper elastic limit for these conditions, or the upper limit of the elastic range, is $15+18=33$ tons per sq. in.

The primitive elastic limit of this steel being 24.9 tons per sq. in., the foregoing means that by varying the position of our

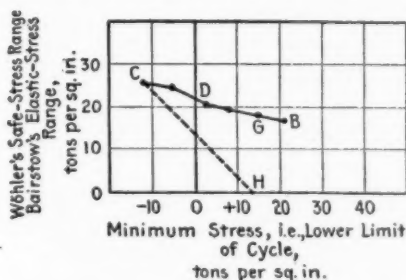


FIG. 1.—The Safe Range and the Elastic Range Coincide, and They Narrow as Their Lower Limit Rises. (Wöhler and Bairstow.)

stress cycles, we can set up a natural tensile elastic limit of 13 tons per sq. in., or of 21.5 tons per sq. in., or of 33 tons per sq. in., the first far below, the second a little below, and the third far above the primitive elastic limit.

Here, to fix our ideas, we have selected the lower limit of the stress range as the fixed point from which the range starts; but we could equally well select the upper limit of the range as the fixed point, or for that matter the mid-length of the range, from which the range stretches both to right and left. In each case we could plot a diagram corresponding to Fig. 1.

Bauschinger's hypothesis, taken broadly, is that the safe range for given material is the elastic range. Bairstow's late results tend to support this hypothesis, as we shall see in Sec-

tion 12, and for purposes of discussion we may take the safe range and the elastic range as one and the same thing.

The elastic range depends on at least three things: first, the position of its own mid-length, decreasing as our service or experimental conditions raise that mid-length from zero upwards, and becoming zero when that mid-length approaches the tensile strength. Or, to vary the angle of vision, the safe range has a width dependent on the lower limit which the attendant conditions of any given case impose. It is further dependent, second, on the composition and, third, on the microstructure of the steel, being wider for high than for low-carbon and presumably for sorbitic than for lamellar-pearlitic steel.

But if it depends on three things it might well depend on a fourth also, the primitive elastic limit as raised by prior overstrain. This question comes before us, "Is prior elevation of the proportionality limit by overstrain effective or fictitious as regards the safe range and the natural elastic limits? Does it move those limits farther apart, or does it raise the safe range as a whole, raising both its upper and its lower limits, or is it inert towards those limits and the width of the safe range?"

If the last is true, then a piece strengthened and given a higher elastic limit by prior overstrain, has for its safe and elastic range only the natural elastic range which existed before such overstrain. This is the elastic range to which the material would return on annealing, an elastic range the width of which, as we have just seen, varies with the position of its lower limit, or of its mid-length, or of its upper limit, whichever we select as a datum point.

Evidence in Section 6 tends to support the hypothesis that such elevation of the elastic limit is fictitious as regards stress cycles between equal tension and compression; let us now ask whether it is as regards cycles wholly in tension.

10. *Is the Elevation of the Proportionality Limit Caused by Prior Overstrain Effective or Fictitious in Widening the Safe and Elastic Range for Stress Cycles which do not Pass into Compression?*—There is a belief that, though (a) the strengthening caused by prior overstrain is fictitious as regards stress cycles

which pass from tension into compression, so that, for instance, the gain of proportionality limit caused by cool forging does not widen the safe range for such cycles, yet (*b*) it is effective for cycles which do not pass into compression, and does widen the safe range for such cycles. To put this belief in other words, whereas a member which has been strengthened by cool forging does not have any greater fatigue strength for cycles which pass from tension into compression than it would have had without that cool forging, it does have greater fatigue strength for cycles which remain wholly in tension, or vary from a tensile maximum to zero.

But this belief implies that when, in varying the position of the stress cycle,—the position for instance of its lower limit,—we carry that lower limit from above to below zero, there must be an abrupt narrowing of the elastic range as that limit passes the zero ordinate, so that the safe and elastic range curve is something like *CDEF* in Fig. 2. But there is no such drop at the zero ordinate, and hence this belief is untrue, if Bairstow's curve is true.

The difficulty is not to be evaded by the hypothesis that, though this effacement of strengthening due to initial overstrain is complete when the mid-length of the elastic range is zero, it becomes progressively less complete as this mid-length shifts to the right, becoming zero when the lower limit of the elastic range is made zero. Even on this hypothesis there should still be a break at the zero ordinate, as in Fig. 2, for the assumed progressive decrease of effacement as the lower limit of the elastic range rises towards zero, would be followed on passing the zero ordinate by a cessation of effacement, which cessation in itself would be represented by a break at that ordinate, as from the branch *CD* to *DB* of Fig. 2.¹ In fact, if anything

¹ That there would be this break, on this hypothesis, may be made clear by Fig. 3, which is comparable with Figs. 1 and 2, *CDB* being the elastic range for un-overstrained steel, and *CD'B'* that which we are seeking for overstrained steel. Remember that we are considering the effect of stress cycles in excess of the natural elastic range on the initial elevation of the elastic limit by prior overstrain, as in cold-rolled and hard-drawn objects. Because this hypothesis assumes that this elevation of the elastic limit widens the elastic range for stress cycles which do not pass into compression, *D'B'* must be above *BD*. Because it assumes that this elevation is not effaced at all by cycles which do not pass into compression, *D'B'* must be parallel to *DB*. Because it assumes that this elevation is effaced completely by stress cycles in which the compression equals the tension, that is, those which have their mid-length at zero, the two curves must coincide at *C*, which is the width of the natural elastic

Bairstow's curves break in the opposite direction at *D*, concave and not convex upwards.

There is, of course, the chance that the decrease of the effacement of the elevation of the elastic limit by prior overstrain may be a continuous one, not reaching zero when the minimum stress of the cycle is made zero, but continuing as the stress cycle moves farther and farther to the right. Conceive, for instance, a music wire kept always under extreme tension, but with that tension passing, at each vibration, through a cycle in excess of the natural elastic range. Such a wire may lose in that stress cycle only a very small part of the elevation of its elastic limit caused by its wire-drawing. So with a cool-forged bridge member kept always in severe though varying

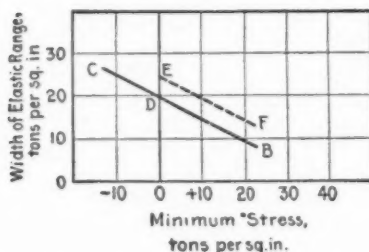


FIG. 2.—Hypothetical Relation of Width of Elastic Range to Stress at Its Lower Limit.

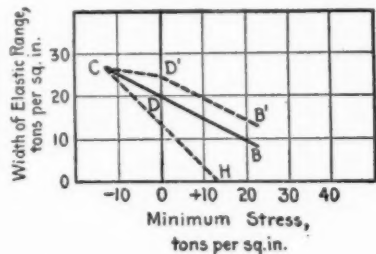


FIG. 3.—Relation of the Elastic-Range Curve of Overstrained to that of Un-overstrained Steel, on a Special Hypothesis.

tension. Under these conditions it might retain a much larger fraction of the elevation of its primitive elastic limit caused by being forged cool, than if, at each stress cycle, the tensile stress fell to zero.

range of un-overstrained steel when its mid-length is zero, as may be seen by drawing the line *CH* at an angle of 45 deg. downwards. Because our hypothesis assumes that there is a progressive effacement of the prior elevation of the elastic limit as the lower limit of the stress cycle is progressively raised from *C* to zero, the curve for overstrained steel must pass from *C* to *D'*, and hence must break at *D'*, convex upwards. *Q.E.D.*

The inference from these considerations is that, if Wöhler's and Bairstow's safe and elastic-range curve is truly a straight line, and if the effects of prior overstrain are completely effaced by stress cycles in excess of the natural elastic range and centered at zero, then they must also be removed, though perhaps to a progressively decreasing degree, as the cycle center rises to and past zero, and thus be in part removed by such cycles even when they are wholly in tension.

Or the effacement of prior overstrain, even by complete stress reversals, may be either incomplete, or extremely slow, or be less and less complete and slower and slower with increasing intensity of the prior overstrain, and with decreasing excess of the actual stress range over the natural elastic range.

Table II assembles a few cases which point towards an effacement, or at least a lessening, of the initial elastic limit, by repetition of stress cycles which do not pass into compression, but vary between zero and a tensile maximum. In

TABLE II.—THE LIMIT OF THE ELASTIC AND OF THE SAFE RANGE FOR CYCLES WHICH ARE TENSILE ONLY IS LESS THAN THE INITIAL YIELD POINT OR ELASTIC LIMIT.

No.	Authority.	Material.	Limit of Elastic Range, tons per sq. in.	Limit of Safe Range, tons per sq. in.	Yield Point or Elastic Limit, tons per sq. in.	Excess of Yield Point or Initial Elastic Limit over Limit of Elastic Range or Safe Range, tons per sq. in.	
						Yield Point minus Limit of Elastic Range.	Elastic Limit minus Limit of Safe Range.
1	Bauschinger	Thomas steel rail	..	<16.4	19.0	..	>2.6
2	"	Mild-steel boiler-plate	..	<16.4	17.6	..	>1.2
3	Bairstow	Axle steel	21	24.9	3.9	...
4	"	Bessemer steel	30	32.0	2.0	...

Nos. 1 AND 2: Taken from Unwin, "Testing of Materials of Construction," 1910, p. 383.

Nos. 3 AND 4: Collected Researches, National Physical Laboratory, Vol. VI, p. 165 (1910), and *Philosophical Trans. Roy. Soc., A*, Vol. 210 (1910).

cases Nos. 1 and 2 the limit of the safe range, and in Nos. 3 and 4 the limit of the elastic range, is appreciably less than the initial elastic limit, which we may suspect had been raised by overstrain, as is so generally the case in steels not purposely annealed to remove overstrain. In Nos. 1 and 2 this "elastic limit" very probably refers to the proportionality limit; in Nos. 3 and 4 it refers to the yield point, but in regard to No. 3 the "material showed no want of elasticity in tension until within a short distance of the yield," so that the initial proportionality limit was almost certainly higher than the limit of the elastic range for repeated cycles.

These cases tend to show that the assertion made by an eminent writer that "innumerably repeated stresses of like character, not exceeding the primitive elastic limit, cannot produce rupture" does not hold true of the initial elastic limit if it has been raised by prior overstrain, though it may be true if we use "primitive" in the sense of the "elastic limit which the metal has in the absence of overstrain."

11. *Phenomena Suggesting that the Prior Elevation of the Elastic Limit Persists.*—The hypothesis that stress reversals, if carried beyond the natural elastic range, efface the elevation of the elastic limit caused by prior overstrain, implies that hard-drawing and other forms of overstrain do not increase the elastic range under such reversals; that for instance, the gold springs of pincenez eye-glasses, the shanks of a dentist's cutting tools, knitting needles, etc., which in use are subjected to stress reversals, are not more elastic because of being worked cold into shape, an implication which is contrary to common observation. We know that they are far more springy, and apparently persistently springy, because of that cold-working, that is, because of that overstrain.

But if any cycles which exceed the natural elastic range of the un-overstrained steel efface the overstrain, then such wire-drawn objects should have no higher elastic limit, and in that sense should be no stiffer as regards the angle they can bend without setting, than like material annealed.

In like manner the hypothesis that stress variations which remain constant in sign efface the stiffening effect of such prior overstrain, would imply that the hard-drawing of wire for bridge cables and for pianos makes it no stronger for resisting its cycles of stress, an implication equally opposed to common observation. We know that hard-drawing increases the stiffness, strength, and springiness, and that this increase persists after very many repetitions of the cycle of stress.

It may be that this persistence of the effects of overstrain during repetitions of stress, both with and without change of sign, is only temporary. Thus Bairstow found that only after some 8000 repetitions of stress between -8.3 and $+18.3$ tons per sq. in. did any permanent set arise which could be detected even by his delicate methods, that is, did an effacement of prior

overstrain reveal itself. Moreover the effacement of such intense overstrain as exists in hard-drawn wire may be even more slow and gradual than that of the very moderate initial overstrain of Bairstow's materials.

At least we have reason here to suspect that the laws of this effacement of prior overstrain are yet to be discovered. Is the evident greater stiffness of the hard-drawn state truly permanent, persisting through an indefinite number of stress cycles, or has it a life, so that it is effaced gradually by such cycles? For the 8000 cycles which were needed in Bairstow's case to develop any detectable loss of elasticity in his initially only slightly overstrained steel, shall we read millions or billions of cycles needed to lessen materially the extreme overstrain caused by wire-drawing? Hoisting cables may well be worn out before the harmful number of cycles has been passed. But with the more permanent bridge cables, is the number of cycles needed to cause serious effacement of overstrain so vast that the life of these cables is not of decades but of centuries?

12. *Circumstantial Support of Bauschinger's Hypothesis* is given by Bairstow's late researches,¹ in which he showed that the elastic range during stress cycles followed a curve of the same family with that of Wöhler's curves of the safe range under like stress cycles. Thus in Fig. 1, the curve *BC*, which represents the width of the elastic range of stress as found by Bairstow for his "axle steel," when plotted to this small scale, coincides with the safe range found by Wöhler for his "untempered spring steel." The corresponding elastic-range curves of Bairstow's other materials and the safe range-curves of Wöhler's other materials also were of this same family, approximately parallel with *BC*.

This resemblance seems to have been interpreted in general as proof that the elastic range and the safe range are identical, and hence as a confirmation of Bauschinger's hypothesis. But it seems to me not confirmation but only strong circumstantial support. Accepting Bairstow's results at their face, they prove that, with varying minimum stress limit, the safe range is parallel with the elastic range; but because his mate-

¹ Bairstow, *Collected Researches*, National Physical Laboratory, Vol. VI, p. 187 (1910); also *Philosophical Trans. Roy. Soc., A*, Vol. 210 (1910).

rials are not known to correspond closely in their natural elastic limits to Wöhler's, they do not prove that these two ranges are identical, though they increase the probability of that identity very greatly. Beyond this, Bairstow's results do not command complete acceptance as quantitatively accurate. They were reached by extrapolation, under difficulties the gravity of which he frankly admits.¹

I disclaim expressly any conclusion as to whether repetitive stress beyond the primitive elastic limit does or does not efface the strengthening effects of prior overstrain. Instead I point out that the indications that it does are sufficiently important to call for thorough study of the question, whether that strengthening is or is not fictitious for services implying indefinite repetitions of stress.

13. *Evidence that the Effects of Simple Overstrain are not Monotropic.*—First, though the tensile stress which causes a tensile overstrain is applied lengthwise, yet in that it causes a reduction of area which is a function of the elongation in any given section, it is in effect transverse too. In each grain, as it elongates, the crystal units slide in a plane parallel to two of their axes, a plane which can only exceptionally be parallel to the pull. The travel of all but an insignificant proportion of these units has a component transverse to the pull, so that some transverse effect of the overstrain is expected with confidence.

Second, a hard-drawn wire is stiff in every direction, for example, against bending, and apparently strong against both tension and compression, because its bending resistance is composed of the tensile resistance of the convex and the compressive resistance of the concave side. If it were weakened compressively just as much as it is strengthened tensilely, then it should be no stiffer against bending than before the wire-drawing, which is untrue. Of course, some quantitative difference between the strengthening in the direction of the overstrain and that in the opposite direction may remain, if not in a drawn wire which in drawing is both compressed laterally and elongated tensilely, at least in a wire torn tensilely without lateral compression.

¹ Bairstow, *Collected Researches*, National Physical Laboratory, Vol. VI, p. 180, paragraphs 5 to 7 (1910); also *Philosophical Trans. Roy. Soc., A*, Vol. 210 (1910).

Third, the familiar properties of cold-rolled metallic sheets suggest pantropy. A sheet cold-rolled in only one direction seems at first as stiff crosswise as lengthwise.

Fourth, cold-twisting raises the tensile strength, that is, the strength in a plane 90 deg. from that of the main distortion.

Fifth, cold-rolling, like wire-drawing, draws the metal out lengthwise; yet in the Lauth cold-rolling procedure at the works of the Jones and Laughlin Steel Co., as investigated by Thurston,¹ it increased the transverse strength by between 50 and 80 per cent, while increasing the tensile strength in a smaller proportion, by between 25 and 40 per cent. It raised the "elastic limit" which was probably the proportionality limit, torsionally, tensilely, and transversely, by between 80 and 125 per cent, and the elastic resilience by between 300 and 400 per cent lengthwise and by between 150 and 425 per cent crosswise.

14. *Evidence that the Effects of Simple Overstrain are Isotropic.*—The foregoing evidence that the effects of simple overstrain are pantropic leaves us more receptive toward the evidence, which I now offer, tending to show that the Brinell hardening effect of tensile overstrain is isotropic.

From a test piece of steel of 0.11 per cent of carbon which had been torn in two in the common tension test, seven rectangular blocks were cut and tested with the results shown in Fig. 4.

In the tension test the deformation is greatest in the necking section where rupture occurs and least in the grip-ends, and this is reflected by the greater hardness at *A* and *B* than in the rest of the measured length *C* and *D*, and by the greater hardness here than in the grip-end *F*. But even in the grip-ends some hardening occurs, as is shown by the greater hardness, 154, at the inner end *a* of the grip-section *F*, than midway of this block, 138-141 at *Fb*, and by the still less hardness at the extreme end, *Fc* 132, where the deformation was least.

Moreover, the surface of the test piece is slightly harder in block *C* (177 at *Cb* and *Cd*) than at the axis (169 at *Ba*, *Dc*, *Ce*, and *Cf*), suggesting somewhat greater surface than axial deformation.

¹ Thurston, "Materials of Engineering," Vol. II, 1898, p. 616.

Making allowance for these two complications, the approach to quantitative isotropy is surprisingly close. Consider the impressions made by balls *G* and *H* sketched in Fig. 4. As the metal flows away from the ball its movement has two components:

f, forward in the direction of the travel of the ball and of the pressure applied to the ball; and

r, radial, in all directions in a plane at right angles to that direction.

But as regards the bar itself four directions may be distinguished, as sketched at *G* and *H*:

<i>n</i> , narrowing	the bar by mov- ing its par- ticles	nearer the axis, as at <i>fn</i> for ball <i>G</i> ,
<i>w</i> , widening		away from " " " <i>rw</i> " " <i>H</i>
<i>t</i> , tensile, lengthening		in the direction of the prior overstrain, as at <i>rt</i> " " <i>G</i> ;
<i>c</i> , compressive, shortening		opposite to that of the prior overstrain as at <i>fc</i> " " <i>H</i> .

If the effects of overstrain are isotropic, making the resistance of the metal to displacement equal in every direction, then the impression made by the ball should be independent of its direction. If they are anisotropic, so that the resistance of the metal to displacement varies with the direction of that displacement, then only by an extraordinary coincidence can impressions *G* and *H* be equal, because the components of the resistance at *G* are the opposite of those at *H*, *G* lengthening and *H* shortening the metal in the direction of the axis of the bar, *G* narrowing and *H* widening it transversely of the bar. Moreover, the component transverse to the bar is parallel to the movement of ball *G* but normal to that of ball *H*, and the component which is axial to the bar is normal to the movement of *G* but parallel to that of *H*.

If such a coincidence should occur with the specific ratio of radial to forward displacement corresponding to a given width of impression, that it should repeat itself with the different ratio corresponding to another width of impression would

be a coincidence in the second degree, antecedently improbable in the second degree.

Yet such exact identity occurs in block *C*; and in blocks *A* and *B* the deviation from identity is within the limits of experimental error. In the axial part of block *C* the longitudinal hardness is the same as the transverse, 169.¹ In the more deformed blocks, *A* and *B*, the correspondingly greater hardness is almost constant, varying only between 186 and 195. In the grip-block *F* the axial hardness decreases from 154 at the more deformed end *a*, to 132 at the less deformed end *c*, the transverse hardness *b* being midway between them, just as it would be if the hardening effect of the overstrain were strictly proportional to the degree of deformation and independent of the direction. Much the same is true of block *D*.

The Brinell hardness is based on the permanent deformation or set under given total stress, and thus is a function, though not necessarily a simple one, of the true elastic limit at which permanent set begins. Hence the present results indicate that the effects of overstrain on the true elastic limit are isotropic.

This conclusion points to a chemical change. If, for instance, overstrain should change a mixture of hydrogen and oxygen into water, its mechanical effects would naturally be isotropic, and so they would be if it brought about any other chemical change, a term which of course may include molecular changes, such as from crystalline to amorphous.

After these tests had already shown clearly that the effects of this tensile overstrain on the Brinell hardness are at least approximately isotropic, I found that Charbonnier and Galy-Achè² in 1900, and Hanriot³ in 1913, had pointed to isotropy, in the case of other metals.

15. *Evidence Misinterpreted as Showing Monotropy of Overstrain.*—From the fact that overstrain in either direction lowers

¹ This longitudinal axial hardness of block *C* was not determined directly. But the longitudinal axial hardness of both blocks *B* and *D*, in their sections which abutted directly against this axial part of block *C*, was 169, which therefore may be taken as applying also to block *C*.

² "Etude sur les Propriétés Physiques et Mécaniques des Métaux," Congrès Internat. des Méthodes d'Essai, Paris Exposition, Vol. I, p. 270 (1900). See also Galy-Achè, *Mémoires, Revue de Métallurgie*, Vol. 10, p. 587 (1913).

³ Hanriot, *ibid.*, p. 595.

the proportionality limit in the opposite direction if determined immediately after the overstrain, some have inferred that it is monotropic, in the sense of strengthening in its own direction though weakening in the opposite direction, inferring for instance that a tensilely overstrained piece, if judged by its proportionality limit, is stronger tensilely but weaker compressively than before. So far as I know this has not yet been shown. On the contrary, such evidence as I have found points in the opposite direction. Thus the lowering of the compressive proportionality limit caused by tensile overstrain has been noted immediately after the overstrain.¹ But here is no true suggestion of monotropy, because the tensile proportionality limit also, if determined at that time, has been lowered simultaneously very greatly, if not indeed to zero.²

It is perfectly true that this tensile limbering is only temporary, and that the tensile proportionality limit thus lowered rises slowly during rest in the cold, and very rapidly on heating. This may have given rise to the idea of monotropy, on the assumption that the compressive limbering is permanent. But for this assumption I know of no evidence. On the contrary, some results of Howard's show that after tensile overstrain the compressive strength recovers during rest somewhat as the tensile strength does, as is shown by the data in Table III.

To explain: a double-refined wrought-iron bar, 2.02 in. in diameter, and 6 ft. 8 in. long, was tested tensilely with the results given in column No. 2. After a rest of four years, test-pieces cut from one of its fragments were tested tensilely and compressively (1) without any treatment other than this rest, column No. 3, and (2) after annealing, column No. 4. The results are complicated by the fact that the compressive failure

¹ Gray, *Transactions, Amer. Soc. Mech. Engrs.*, Vol. 18, p. 706 (1899) and Bairstow, *Philosophical Trans. Roy. Soc., A*, Vol. 210, p. 35 (1910), and Collected Researches, National Physical Laboratory, Vol. VI, p. 165 (1910), traced continuous stress-strain curves in alternate tension and compression. After the stress had been lowered to zero from a tensile maximum beyond the elastic limit, and compression was then applied, the new stress-strain locus was curved from its beginning, so that the compressive proportionality limit was zero. So, too, on releasing stress which had been beyond the compressive elastic limit, and again applying tensile stress, the new tensile stress locus was curved from its beginning, the tensile proportionality limit thus being zero. Thus overstrain in each direction effaces the proportionality limit in the opposite direction.

² Muir, *Philosophical Trans. Royal Soc., A*, Vol. 193 (1899).

was in each case by triple flexure because of the slenderness of the compressive test pieces, so that the tensile strength played an important part in the observed compressive strength. But, taking these data at their face value, and admitting that, if the test had occurred immediately after the overstrain, the proportionality limit in each direction would have been negligibly low, there has certainly been a recovery of the compressive elastic strength during the four-year rest.

TABLE III.—TENSILE OVERSTRAIN PLUS REST STRENGTHENS WROUGHT IRON BOTH IN TENSION AND COMPRESSION. J. E. HOWARD.¹

Properties.	Initial.	Overstrained in Tension, then Rested 4 Years.		Gain on Overstrain plus Rest, in per cent of the Initial.
		Not annealed.	Annealed.	
1	2	3	4	5
TENSION TEST.				
Tensile strength, lb. per sq. in.	50 156	73 160	51 560	45.86
Elastic limit, lb. per sq. in.	29 375	73 160	26 000	149.05
Elongation, per cent.	22.7 in 80 in.	5.0 in 5 in.	26.2 in 5 in.
Reduction of area, per cent.	43.0	23.0	35.6
COMPRESSION TEST.				
Compressive strength, lb. per sq. in.	58 600	28 550	105± ^a
Elastic limit, lb. per sq. in.	20 000	15 000	33± ^a
Permanent set in 10 in., in.	0.0199	0.0146

¹" Tests of Metals," Watertown Arsenal, 1888, p. 835.

^a These numbers are based on the assumption that the initial properties are identical with those found after annealing. That this assumption, though probably only an approximation of the truth, is a close enough approximation for our present purpose, is indicated by the closeness of the tensile properties after annealing to the initial ones.

If we assume that the annealing restored the iron approximately to its condition before the initial tensile overstrain, then, as shown in column No. 5, the net effect of that overstrain plus the four-year rest has been to strengthen and stiffen the iron both tensilely and compressively, though the compressive gain is less than the tensile.

This test, then, on its face goes to show that the effects of tensile overstrain are of like sign in all directions, that is, pan-tropic, though not necessarily isotropic.

The proportionality limit seems to be more anisotropic than the tensile and compressive strengths, that is, the reduction of this limit in a given direction by overstrain in the opposite direction seems to be recovered during rest less fully than the simultaneous reduction of compressive or tensile strength. If we regard the stress-strain curve as approximately the locus of the elastic limit during the overstrain of the tensile test, this contrast becomes the less surprising. For in this case the observed tensile strength is only the maximum elastic limit to which the overstrain in the tension test itself raises the piece as a whole, in spite of the simultaneous reduction of its sectional area. In this view any elevation of the elastic limit as observed in the tension test means an anticipation, before the test, of the elevation which would have occurred in the test itself, an anticipation which theoretically need not lead to any increase in the observed tensile strength, being then an anticipation solely and not an addition. The fact that prior overstrain, as in cold-rolling, commonly raises both elastic limit and tensile strength, but the former more than the latter, can be interpreted as meaning that the elevation of the elastic limit which that prior overstrain causes is partly an anticipation of the elevation which would occur in the tensile test itself, and partly an addition to it.

Acknowledgments.—This investigation has been carried out in part under a grant from The Carnegie Institution of Washington. I have to thank my assistant, Mr. A. G. Levy, Professor James S. Macgregor, and Mr. F. Miller, of Columbia University, for the Brinell hardness and density determinations.

ADDENDUM (*by letter*).

The evidence in Section 10, tending to show that the elevation of the elastic limit by overstrain is fictitious as regards the endurance of stresses between zero and a tensile maximum, was defective in lacking direct data as to the proportionality limit of Bairstow's materials Nos. 3 and 4 of Table II. Since then I have received the missing data from Mr. Bairstow, showing that the proportionality limit of his axle steel was 24.5 tons per sq. in. as received from the maker, and 22 tons per sq. in. after annealing at 600° F. The limit of the elastic range for such

stress cycles for this steel Mr. Bairstow found to be 21 tons, or 3.5 tons less than the proportionality limit of the material caused by rolling, and removed by annealing at 600° F. Here, then, is direct evidence tending to show that the elevation of the proportionality limit, caused by the mechanical overstrain of the rolling process in the hands of the maker, is fictitious as regards the endurance of stresses varying between zero and a tensile maximum.

The data assembled in Table IV, received about September 26 from a very trustworthy source, show that the great elevation of the yield point caused by cold-rolling may be effective for repetitions of stress to rupture even under relatively small fiber

TABLE IV.—PRIOR OVERSTRAIN MAY EITHER LESSEN OR INCREASE THE ENDURANCE.
ENDURANCE OF ALTERNATING EQUAL AND OPPOSITE STRESSES BEFORE RUPTURE (IN HUNDREDS OF THOUSANDS).¹

Deflection, in. . .	$\frac{15}{16}$			$\frac{13}{16}$			$\frac{11}{16}$			Tensile Strength, Aver., lb. per sq. in.	Yield Point, Aver., lb. per sq. in.
	28 100			24 300			20 600				
Fiber stress, lb. per sq. in. . .	Repetitions of stress before rupture, in hundreds of thousands.										
	Max.	Min.	Aver.	Max.	Min.	Aver.	Max.	Min.	Aver.		
Cold-drawn.	2.3	1.0	1.6	3.4	2.5	2.0	4.3	3.1	3.8	71 900	68 800
Cold-rolled.	5.8	4.7	5.4	20.3	12.5	15.8	244.0	44.7	121.5	66 800	63 400
Turned.	2.9	1.6	2.3	9.3	3.7	6.7	24.2	5.1	13.4	62 400	36 900
Cold-rolled ÷ Turned	2.35	2.36	10.1	8.67	9.07

¹ For instance, the maximum number of repetitions before rupture endured by the first cold-drawn bar was 227,897 = 2.3 hundred thousand.

stresses. Not only is the endurance much greater after than before cold-rolling, but what is more important this increment is the greatest when the fiber stress is the smallest, and therefore the nearest to service conditions.

Nine $2\frac{9}{16}$ -in. round bars from one heat of basic open-hearth steel of 0.22 to 0.27 per cent of carbon and 0.64 to 0.69 per cent of manganese, each 45 ft. long, were each cut into three 15-ft. pieces. These pieces were then reduced to a diameter of $2\frac{1}{2}$ in., (1) by cold-drawing one from each bar in one draft; (2) by cold-rolling one from each bar in from 26 to 32 passes; and (3) by turning one from each bar in a lathe. From each of these 27 pieces a 30-in. specimen was then cut and tested tensilely, the section having first been reduced to a diameter of $2\frac{1}{4}$ in. for a length of 12 in. to localize rupture there. Since this removed

the outer surface of the bars, the tensile tests do not represent accurately the properties of the bars as a whole.¹ This of course does not affect the endurance tests.

For the endurance tests a 12-ft. length from each bar was rotated at the rate of from 387 to 437 turns per minute while supported at both ends and bent with a deflection of from $\frac{11}{16}$ to $\frac{15}{16}$ in., caused by means of a load applied at the middle of its length.

The endurance of the cold-drawn bars was even less than that of the turned ones. This may well represent unfavorable conditions of cold-drawing, but at least it shows that a cold-drawing which nearly doubles the yield-point stress instead of increasing may greatly lessen the endurance.

The endurance of the cold-rolled bars was a little over double that of the turned bars under deflections of $\frac{15}{16}$ and $\frac{13}{16}$ in., but, strange to say, it was nine times as great as that of the turned bars under a deflection of $\frac{11}{16}$ in. A surprising result is that the ratio of the endurance of the cold-rolled to the turned bars is almost the same for the bars with the greatest endurance and with the least endurance as for the average under this deflection.

The variations in endurance are indeed very great, as they often are in other investigations. That they are not due to irregularities in the cold-working is shown by their occurring also in the turned bars. There is no corresponding variation in the tensile properties. The endurance seems to be very capricious, in the sense of being influenced greatly by minor conditions easily escaping notice.

The increase of endurance is naturally to be referred to the inferred elevation of the true elastic limit, of which the elevation of the yield point is a concomitant. But the loss of endurance caused in these cases by cold-drawing suffices to show that while this elevation may increase the endurance, it may also lessen it. This lends weight to my suggestion that, till the conditions of overstrain which increase the endurance have been distinguished from those which lessen it, it would be well that reception tests for services implying indefinite repetitions of widely varying stresses should be applied to specimens annealed at 600° C. in order to remove the now deceptive effects of overstrain in manufacture.

¹ The tensile properties of cold-drawn steel are affected greatly, at least in some cases, by turning off the outer parts of the drawn bar. Thurston did not find this true of cold-rolled wrought iron. (The Author, "The Metallurgy of Steel," p. 216.)

DISCUSSION.

Col. Dunn. COL. B. W. DUNN.—Mr. President and gentlemen, I have a simple problem in this line that has given me some food for thought. I should like very much to get some advice on it. It is part of my duty to fix the specifications for the tests of steel cylinders to transport compressed gases. Following the English custom, those specifications were fixed some years ago to require each cylinder in a manufacturer's plant to be tested as near as practicable to the elastic limit of the cylinder. In order to avoid passing the elastic limit, the cylinder is placed in an ordinary water jacket. The total expansion under the prescribed test pressure is measured, and the amount of permanent expansion remaining after release of stress is noted. The percentage of this expansion remaining permanent must not exceed 10 per cent of the total expansion.

As our experience accumulated in testing cylinders under these conditions we found that taking, for example, a cylinder to be tested at 3000 lb., and finding the percentage of permanent expansion to be in excess of 10 per cent, all we had to do was to relieve the stress and apply it again. The percentage remaining permanent the next time would be materially less. Sometimes only one reapplication of the stress was necessary. Two or three applications would be certain to drop this percentage from, say, an extreme case of 25 per cent down to less than 10 per cent.

Now, the practical question is, did we hurt the cylinder by permitting the stress to be applied successively three or four times until the cylinder was able to pass the test? There is no reversal of stress of the kind mentioned by Mr. Howe, and in the use of the cylinder there is no great appreciable repetition of stress. A cylinder may be charged, shipped, emptied, and sent back, at most, half a dozen times a year; and possibly twenty years would measure the reasonable life of the cylinder. I should like to hear comments from Mr. Howe and others.

Mr. Howe. MR. HENRY M. HOWE.—If there is a ductility test required for these cylinders, I think it should be made after this expansion

you speak of, because the expansion itself will reduce the ductility. If you determine the ductility before this artificial expansion and then submit your cylinder to this artificial expansion, you have not the same ductility you started with and your first test is deceptive. **Mr. Howe.**

COL. DUNN.—The ductility is determined by what is called a "crushing test" of a sample cylinder; the metal is not allowed to crack before the crushing wedges have approached to a distance measured by six times the wall thickness of the cylinder. Usually, there is a margin of ductility in the cylinders to pass that test; so that my question assumes that whatever hardness results from this slight overstrain, will be taken care of by the margin in the ductility test. **Col. Dunn.**

MR. HENRY HESS.—Considering the hydraulic cylinders referred to by the previous speaker, it may very well be questioned whether the observed deflection really represented a permanent set. To determine that it would have to be known how many measurements were taken and at what time intervals and what those measurements were quantitatively. Obviously no "permanence" can be established for any phenomenon without repeated observation through a sufficient time range. While the "set" of the hydraulic cylinders undoubtedly existed as measured, it does not at all follow that it amounted to a "permanent set" of the same amount. **Mr. Hess.**

It may be that some experience that I gained in the manufacture and use of ball bearings will serve to throw some light on the questions involved. A ball-bearing journal is loaded on one side; it follows that the balls in passing from the unloaded side to the loaded side receive and are released from load. The higher the speed of rotation the more often does this change from no load to full load occur in a given unit of time. Experience has shown that failure occurs very much sooner at high than at slow speed. It might be reasoned from this that the life of such a ball is a function of the total number of times it is loaded, but further analysis of the experience records shows that there is a functional relationship between the life of a ball and the *time elapsing between successive loadings*. Thus the same bearing running at slow speed will show a far greater number of loadings for a ball than at high speed. It was apparent that a ball's life

Mr. Hess. was prolonged if it was allowed to recover from the deflection between loadings. This was absolutely proven by many experiments in which all such disturbing elements as impact and contact of ball on ball were carefully eliminated. These experiments were directed toward determining the quantitative relationship of ball size, load, deflection and time rate of successive loading. Though these various factors were not fully found and approximate quantities only were established, yet the law may be considered to have been proved, that the life of any element of mechanism, subjected to recurring stress, will be greatly prolonged if time be given for full recovery from stress deformation between successive stressings. Slight stresses, so quickly repeated as not to permit of recovery, may be far more dangerous than much heavier stresses occurring at longer intervals. It is not the amount of stress, it is not the amount of deformation that is of chief importance, but it is the repetition of stress before complete recovery from deformation, that is killing. It is generally accepted as correct that to secure long life, stresses must be kept within the elastic limit; that is true, but it is not sufficient; it should be supplemented "and stress must not be repeated before complete recovery from deformation due to the previous stress has taken place." Unfortunately, very little information indeed is available on the time rate of recovery from deformation.

Taking up another subject more in the line of Mr. Howe's original remarks: About a week ago I was present at the Summer Meeting of the American Society of Mechanical Engineers at St. Paul. A report was submitted by a committee on boilers which included some recommendations regarding boiler plate that were much objected to by some makers of such material. It was objected that the physical qualities, such as ultimate strength and elastic limit, must and did vary greatly with the gage to which rolling was carried as well as with the unavoidable variations in temperature between successive plates as they were submitted to the last passes, and that those resultant fluctuations in physical qualities could not be kept within the tolerances recommended by the committee. Under the circumstances it seemed to me the rational thing to reduce all test specimens to that condition under which the effects of those unavoidable variations of working were removed. That could be done by a

thorough annealing of all such specimens, followed by a uniform **Mr. Hess.** heat treatment. That would eliminate the inadvertences of variable working, always assuming that all of the working had been held within the limits of good rolling mill practice. The physical tests of specimens so treated would be concordant and representative of their real quality to an extent that would permit of quite small tolerances. The committee met with quite a little opposition; it was suggested that it ought to reconstitute itself as a joint committee, adding to itself officially members of other interested technical bodies, such as the American Society for Testing Materials. The committee professed its willingness to hear members of such other societies as witnesses, but voted down my suggestion that such other societies sit with it in the jury box. That attitude is an unfortunate one and totally at variance with the broad policy outlined by our President to-night of cooperation among the various technical societies. In line with that policy I recommend that our Society, through its governing body, take cognizance of that A. S. M. E. boiler committee and its work, and that if and when our Society is called upon to appear on the witness stand it refuse, and demand and insist upon being called into the jury box.

Mr. G. E. THACKRAY.—Referring to the paper of Mr. Howe **Mr. Thackray.** with respect to the influence of cold rolling or cold drawing, I have no absolute facts or figures to present, but it might be interesting to note one or two little things in passing. For example, cold-rolled steel has been used for a great many years for axles for street-railway cars. Nobody can say today, possibly, that in the light of our present knowledge of heat-treated axles of carbon steels or special alloy steels, that the ordinary, old-fashioned, cold-rolled axle is the best; but the experience, at least, of many roads in this country has shown that those axles, with a mileage of many hundred thousands, have stood up to their work as well as or better than some ordinary steel. We who make steel, however, know this to be a fact, at least qualitatively, if not quantitatively; that all steel which has been subjected to work, whether hot or cold, is stressed internally more or less, and that it obtains some of its properties to a certain extent from that very work.

Another and somewhat similar example is that of the hard-

Mr. Thackray steel or so-called plow-steel wires which are used in elevator and other similar cables. In my neighborhood we use ropes of somewhat larger diameters, 2 in. They last in service maybe two or three years. In that length of time they have repetitions of stresses running into the millions; just how many millions I do not know; but our experience has taught us that at the end of their service, if we take those ropes, caliper the separate wires, compare their effective with their original areas, if it were not for the numerous breakages due to the service, and the wear which is on the outside, the wires themselves appear to be substantially as strong as they were when they went into that rope.

Another thing: of course, we all know that any steel, either hot or cold, which has been subjected to stress may, by a period of rest, reassert its molecules and may possibly have somewhat different qualities after that reassertment has taken place. It has been known that certain complicated forms of steel have such internal stresses that they almost break themselves. Annealing, as we all know, is the thing to which we resort to overcome those stresses.

While the subject which Mr. Howe has presented is a most interesting one and one which I think might be well followed out, experience, at least, has taught in a certain measure that there is not a very great difference, as time passes, in materials of that character; but they do hold their qualities, at least, until they are worn out or thrown away from some other causes than the loss of the qualities which they had in the first instance, and which were given to them by cold-rolling or hammering.

Referring now casually to the remarks of the last speaker, who takes exception to the fact that a $\frac{1}{2}$ -in. plate might have qualities different from those of a $\frac{1}{4}$ -in. plate, a fact which has been well known to users and makers of plates for many years, and who suggested that we might better put that material back into its original condition: that seems to me so out of harmony with everything steel manufacturers have been doing for many years that I cannot quite understand it. Why do we cast ingots, hammer them, bloom them, roll them, except to give them the qualities which you gentlemen want? We certainly cannot put them back in their primeval condition and say, "Let us see what their qualities are in that condition."

Nobody here knows better than Mr. Howe what would be said if we should give you a steel ingot or something with the structure of a steel ingot in place of a plate. We should recognize what has been done with the material when it has been worked by rolling or hammering. Mr. Thackray.

MR. HESS.—It seems that my suggestion that the boiler plate material in question should be put back as nearly as might be into its primal condition has been misunderstood. The suggestion that I sought to convey was that test specimens cut from the plate should be submitted to such treatment as would insure the elimination of the variations due to all legitimate accidental variations in good mill practice, to the end that such a quality specification as that of the committee that ultimate strength should be within 42,000 to 47,000 lb. per sq. in., might not meet with the active opposition of steel makers, the previous speaker among them. Some method of test practice should be evolved which will permit of a comparison of the real quality of a material independent of the overlying and obscuring influence of legitimate and unavoidable variations in good mill practice. Mr. Hess.

MR. F. S. SLOCUM.—The firm I represent are manufacturers of cold-rolled steel, and after Mr. Howe prepared his paper, he wrote to us asking for some information. It may be of interest to state that under supervision of his representatives, we are now making a series of tests which will probably throw some useful light on this subject. Mr. Slocum.

MR. J. S. MACGREGOR.—With the idea of discussing that portion of Mr. Howe's paper under Section 8—"Should Engineering Specifications for Unhardened Reverse-Stress Objects be Based on Annealing at 600° C.?"—which suggests that by such annealing we would obtain an elastic limit more nearly representing that which the steel would have when subjected to numerous stress reversals, I obtained from manufacturers of cold-drawn steel several samples of bars for testing purposes. Mr. Macgregor.

These bars were said to be steel containing 0.4 to 0.5 per cent of carbon and they were of sufficient length to permit of obtaining test specimens, when cut in two. One half were tested as received, the other half were annealed to 600° C. and then tested. Two lots, one having been cold-drawn for somewhat over a year and the other for but several weeks, were tested.

Mr. Macgregor.

I had two objects in view in conducting the tests: first, to determine the effect upon the elastic limit of drawing the material at $600^{\circ}\text{C}.$; and, second, to determine the effect of age upon the elastic limit and ultimate resistance of the material. Table I gives the results obtained, all values being averages of three tests.

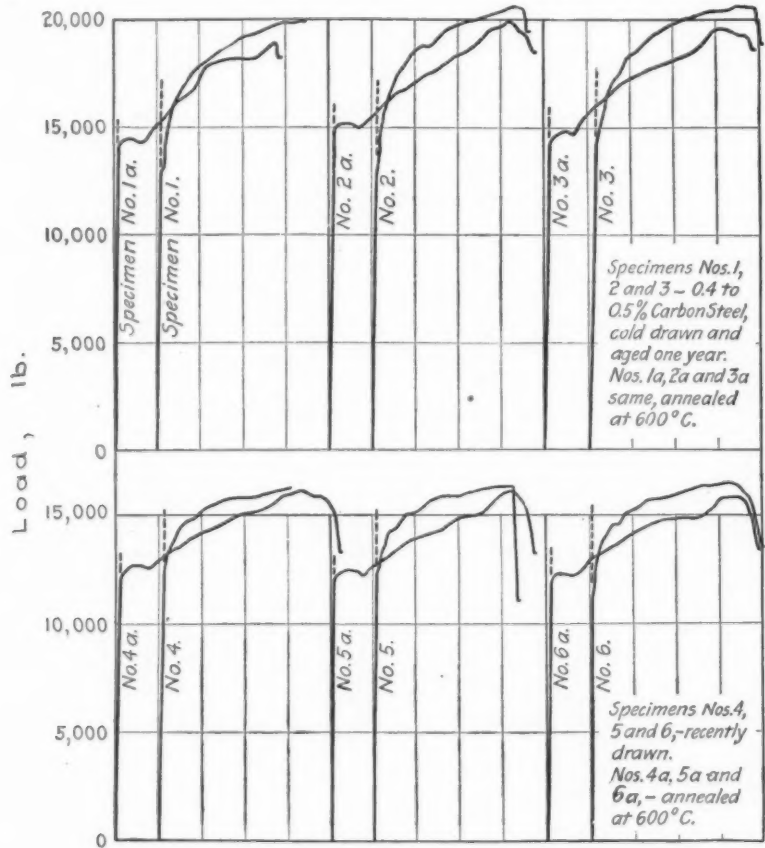


FIG. 1.

A comparison of the above values shows, at the age of one year, a slight increase in the elastic limit and a decrease in the tensile strength; and, at the age of 2 weeks, a slight decrease in both of these values due to the annealing. In both cases there is, as one would expect, an increase in both the percentage of

elongation and reduction of area. These results tend to convince **Mr. Macgregor.** one that annealing cold-drawn material at 600° C. has but a very slight effect upon the elastic limit.

In reference to the effect of age upon the elastic limit, it must be stated unfortunately that from a comparison of the tests upon the two sets of bars in the annealed state it is evident either that their carbon content, or else the heat treatment they had been subjected to before drawing, was not the same, thus making comparison in the above respect impossible.

TABLE I.—VARIATION OF PROPERTIES OF STEEL BAR WITH AGE AND HEAT TREATMENT.

Age of Bar.	How Treated.	Tensile Strength, lb. per sq. in.	Elastic Limit, lb. per sq. in.	Elongation in 4 in., per cent.	Reduction of Area, per cent.
1 year.....	Unannealed.....	104 173	68 277	13.2	29.3
1 year.....	Annealed.....	97 273	73 893	16.5	34.3
2 weeks.....	Unannealed.....	86 973	62 373	13.1	41.6
2 weeks.....	Annealed.....	83 161	61 517	19.1	47.7

I have the autographic records taken from all bars tested, which I submit to you for inspection (Fig. 1). It is interesting to note that due to annealing, the amount of deformation in the material at the yield point is very much in excess of that recorded when they are not annealed. Annealing the bars therefore would enable one to determine the yield point by the drop-of-beam method with accuracy, whereas this determination for the material in the unannealed state is at best very unsatisfactory.

MR. J. E. HOWARD (*by letter*).—Thanks are due **Mr. Howe** **Mr. Howard.** for presenting a paper of the deepest interest, and characteristically handling a subject which lies at the very foundation of the mechanical treatment of metals, and pertains to the maintenance of their structural integrity. Questions are dealt with which are intimately associated with problems having to do with the ultimate endurance of metal under the action of stresses of different kinds, and in defining conditions indexical of the internal state of the metal, and those internal relations which exist after their initial state of repose has been disturbed.

Mr. Howard. Considering the importance of the subject, it must be admitted that there are but meager data of a reliable character to be found bearing upon it. This is a matter of more than passing regret, in view of the volume of testing which has been done, and is constantly being done. No systematic study appears to have been conducted along the lines suggested by Mr. Howe's paper, but therein are found suggestions for a great field of research work, the scope of which is of such extent, that some national institution should eventually be organized to conduct fundamental inquiries of this kind. The logical amplification of the thoughts expressed in the paper leads directly to the acquisition of information in which the limiting stresses that metals are capable of sustaining shall be defined. To this end such results would tend.

The idea of an overstrain carries with it the thought that some permanent change in shape results from a force applied, the effects remaining after the overstraining load has been removed. In order to have a case of overstrain it seems necessary to restrict the force to a simple one, acting in one direction, or at least not having more than two forces simultaneously applied. In the case of three forces of equal magnitude acting at right angles to each other, it is difficult to see how permanent distortion can be effected.

In the case of compressive loads applied in three directions, experimental trials have failed to show any change in the properties of metals after exposure, or resulting from exposure, to loads several times their normal elastic limits, as defined when loads were applied in one direction only. No changes in shape nor in density resulted from the compression loads referred to. Tensile stresses do not admit of being applied simultaneously in three directions, but if they could be so applied no permanent distortion would be expected until the rupturing load was reached.

Local overstraining introduces internal strains which admit of study and definition, measuring the resilience of the metal when the internal strains are released. An indenting load applied to a portion of the surface of a piece of steel introduces internal strains, the effects of which on the adjacent metal may be determined. But the method employed in such a case has reference

to the elastic recovery of the affected metal, and does not take up the case of further distortion of the metal in any direction. Nevertheless, elastic recovery is one of the consequences of an overstrain. Mr. Howard.

An overstraining load which affects the full cross-section of the metal uniformly, or as nearly so as practicable, presents a case in which the elastic resiliences may be observed, and certain of the effects of overload determined. The immediate effect of such an overload is noticeable in the change in the value of the modulus of elasticity of the metal; an effect from which there is partial or complete recovery in the course of time.

The effect of such an overload may be to cause a zone of increased rigidity for loads next higher than the overstraining one. This is the marked effect of overstraining certain puddled irons. The zone of increased rigidity appears to increase with lapse of time, and so far as known remains permanent over many years, no changes in temperature of an annealing nature having been experienced in the meantime.

The effect of an overload on the range of the resilience of the metal in plus and minus directions is noticeable. The range in resilience, or the sum of the resiliences in tensile and compressive directions, is lessened by overstraining in either direction. That is, permanent sets are earlier introduced in overstrained steel, when the stresses are reversed, than in the primitive state of the steel. While this is the case in reference to the elastic movements of the steel, nevertheless the freedom of flow under higher loads is lessened. Herein we recognize two distinct effects resulting from overstrain, one pertaining to the elastic properties of the metal, the other having reference to the extension of the steel for loads above the primitive elastic limit.

A general stiffening of the metal seems to result from previous overstraining. As to whether such stiffening occurs equally in all directions, it is felt that experimental data sufficient to definitely indicate, are lacking.

A vital question pertaining to this branch of the subject is whether an overload causes an actual interior fissure in any or in all cases. Certain effects of overstrain may be removed by annealing, but an interior fissure necessarily would not be repaired. The magnitude of the overstraining load and the grade of the

Mr. Howard. steel might be factors in the case. The cumulative effect of certain repeated stresses is well known, and is exemplified in the rupture of all grades of steel without the display of an appreciable amount of ductility, regardless of the primitive ability of the metal to display extension, in the usual tension test.

Then again, there are examples of steels enduring a prolonged stress without rupture; such stress equal to several times the sum of the two stresses which have effected rupture by repeated alternate loads of tension and compression.

It is to be hoped that experimental research will be inaugurated along the advanced lines which Mr. Howe's important paper has indicated. The time is certainly opportune for directing research work along channels which will give promise of yielding fundamental data upon the properties of the materials of construction. A very liberal volume of data are now available pertaining to the primitive properties of metals in respect to the four features, elastic limit, tensile strength, elongation and reduction of area, and it is further realized that these values in the same material admit of wide variations according to treatment. The significance of those properties and their variations should be ascertained. The author's paper certainly treats of fundamental questions necessary to be known in the proper use of metals.

Mr. Howe.

MR. HOWE (*Author's closure, by letter*).—Mr. Hess's results, showing that the number of repetitions of stress endured increases with the time between repetitions, form a welcome contribution to our knowledge. No one understands better than he the difficulty of excluding complicating conditions, such as the synchronism of the frequency of repetition with the natural period of vibration of the mechanism, the effect of the momentum of the moving parts, and the possible rise of temperature, which may be considerable internally even if the surface is cooled artificially. The question whether there is a real time factor which affects endurance is not settled, and such cases as he cites are to be welcomed.

The proposal to anneal boiler-plate test specimens before test would seem to be especially applicable to marine boilers, for instance, those of vessels in which the vibration is extreme, and the stress cycle therefore wide.

The objections raised to annealing before test probably rest on other considerations. If I understand the boiler conditions, the objection to annealing is that, because the plates are to be used in the boiler without annealing, we wish to know their actual properties, and that to anneal them before testing them would mislead us. Does the reason which I gave for annealing objects, which are to be exposed to many millions of repetitions of stress, apply to stationary land boiler plates? The question which I raised was whether exposure to such an enormous number of repetitions of stress does not efface the effects of cold working. The reason why the number of repetitions which a car axle undergoes is of this order of magnitude is that it rotates many times per minute for several years. But in the case of stationary land boiler plates, I should question whether, even if we take account of breathing and like effects, the number of important variations of stress can reach such an order of magnitude during any probable life of a boiler. Further, the tension test of the boiler plate is in large part to throw light on the power of the material to undergo the abuse in assembling and use, such as punching, drifting, and caulking. A plate finished too cold, and on that account unable to endure this abuse, would not disclose its unfitness if annealed before the tension test.

It is to be hoped that Mr. Macgregor can give fuller information about the previous history of his specimens. His finding that annealing has little effect on cold-drawn steel may perhaps represent some special conditions, for there is a very large mass of evidence showing that annealing does remove the effects of overstrain. Thus, the nearly complete removal by annealing of the effects of wire drawing, not only on steel but on the metals and alloys in general, is perfectly established. Thurston's data showed that the effects of cold rolling on wrought iron are in like manner removed practically completely by annealing.¹ Goerens's data show that 600° F., at least for low-carbon material, is quite high enough to complete this annealing.²

Next, the initial properties of these nominally cold-drawn steels of 0.40 to 0.50 per cent of carbon are about what they should have without cold drawing. Thus the tensile strength of a

¹ Compare with Howe, "Metallurgy of Steel," 1890, p. 210, Table 100.

² Goerens, Carnegie School, *Memoirs*, Iron and Steel Institute, Vol. 3, p. 383 (1911).

Mr. Howe. basic open-hearth steel of 0.50 per cent of carbon and 0.35 per cent of manganese, when hot rolled should be about 87,000 lb. per sq. in. or practically that of his "2-weeks" steel when nominally cold drawn; and that of such a steel with 0.60 per cent of manganese should be about 96,000 lb. per sq. in. or practically that of his "1-year" steel. On their face his data suggest that his specimens had not been "cold drawn" in the usual sense of that term,—that is, of undergoing such reduction as to increase the tensile strength and especially the elastic limit very greatly,—but that they had merely been given a very slight reduction for the purpose of smoothing their surface.

A reasonable degree of cold drawing, as for instance, a reduction of $\frac{1}{16}$ in. in a bar 1 in. in diameter with say 0.30 per cent of carbon, may raise the tensile strength by from 20,000 to 40,000 lb. per sq. in. Therefore, if Mr. Macgregor's bars had really been cold drawn severely we should have expected them to be very much stronger initially than he actually finds them.

Mr. Howard's support of my hope, that the investigations needed to supply the answers to my questions as to the appropriateness of annealing at 600° F., objects which are to undergo many millions of reversals or repetitions of wide stress cycles, is extremely welcome. Such investigations are needed to change our present so-called factors of safety into true factors of safety instead of factors of ignorance. There was reason to hope that at Teddington in England and at Grosslichterfelde West in Germany these investigations would be undertaken, but when peace comes the energies of these institutions may be diverted into other directions.

The matter has been taken up also at one of our American universities in such an extremely intelligent way that we may hope for a firm answer to these questions. The preliminary results which have been sent me go to show that, though under relatively great fiber stress, the strengthening caused by overstrain (cold drawing) does increase the endurance, so that this strengthening is effective and not fictitious, as the fiber stress decreases and approaches such a stress as is actually used in practice, the benefit caused by overstrain decreases progressively, and in such a way as to raise fears that, with what may be con-

sidered a fair normal working fiber stress, the strengthening **Mr. Howe.** caused by overstrain is in large part fictitious, and perhaps wholly fictitious.

Mr. Howard¹ reminds us that his experiments went to show that pressure unaccompanied by deformation does not affect the tensile properties, which is exactly in line with the present theory that overstrain acts primarily through making part of the metal amorphous. Hanriot² has found that the hardness of seven metals and alloys which he tested increased, and in some cases very greatly, when thus submitted to hydrostatic pressure, which did not deform them "sensibly." Unfortunately, the description of his apparatus does not show clearly that his specimens cannot have undergone deformation, and "sensibly" is rather a vague way of indicating the degree of deformation.

¹ *The Railroad Gazette*, Mar. 27, 1908, p. 439, and *The Railway Age*, Mar. 20, 1908, p. 425.

² *Revue de Metallurgie*, Memoires, Vol. 10, p. 602 (1913).

MAGNETIC HABITS OF ALLOY STEELS.

By J. A. MATHEWS.

SUMMARY.

The relationship between magnetic and physical characteristics of steel and its alloys has received much attention. No definite laws expressing this relationship have been found, although broadly considered there are similarities between magnetic and physical behavior which are very striking.

In this paper, after briefly outlining the methods and scope of the work and describing the apparatus and units employed, the following subjects are dealt with:

1. The suggestion of a new magnetic unit of magnetic hardness, B_r/H_c ; that is, the ratio between residual density and coercive force is used to indicate magnetic hardness in a way similar to the use of μ to indicate permeability.
2. The relation between hardness as measured by the Brinell and Shore methods and magnetic hardness as indicated by B_r/H_c is discussed. It is shown that in hardened alloy steels the magnetic properties vary with the cross-section, but in some cases, the magnetic hardness increases as the section increases while in other cases the reverse is true, and in some alloy steels either condition may be had, depending upon the manner of quenching.
3. It is shown that many alloy steels give lower induction, lower residual density and higher coercive force when quenched in oil than when quenched in water; the corresponding physical hardness test showing, as would be expected, that the water quenching confers the greater hardness.
4. The effect of drawing the temper of hardened alloy steels is discussed and it is shown that for equal drawing temperatures the magnetic properties of oil-hardened pieces are most affected, while as to the physical hardness the water-hardened pieces are affected to the greater degree.

MAGNETIC HABITS OF ALLOY STEELS.

By J. A. MATHEWS.

The magnetic behavior of alloy steels presents a most fascinating field for investigation; the reason, perhaps, may be found in the elusive way in which magnetic data baffle our attempts at coordination with other physical properties, or in the persistent frequency with which the unexpected happens. Let us recall, for example, the time when most of us studied physics. The metallic elements were sharply divided into para-magnetic elements, those conducting the magnetic lines of force better than air, and di-magnetic elements, non-magnetic or only feebly so and conducting the lines of force less readily than air. In the former group were iron, nickel and cobalt, also called the ferro-magnetic elements, and most of the remaining metallic elements were classed as di-magnetic.

The peculiar properties of the lodestone were known before the Christian era and from the time Doctor Gilbert published his "De Magnete," in the year 1600, to the present time, magnetic investigations have been numerous.

It was not until the end of the nineteenth century, however, that it was discovered that certain alloys of iron and nickel were non-magnetic under ordinary conditions, and a little later it was learned that they could be made magnetic by cold treatment in liquid air, rather than by heat treatment.

Early in the twentieth century, Heussler published his celebrated researches upon magnetic alloys of so-called non-magnetic elements, such as manganese, copper, tin and aluminum, and these in turn may be rendered non-magnetic by special treatment.

The production of non-magnetic alloys from ferro-magnetic elements, and magnetic alloys from non-magnetic elements, has stimulated research on magnetism during the past decade.

This Society has had a committee for the investigation of the magnetic properties of iron and steel for more than 12 years. The author has engaged in such work for a considerably longer time, and has been a member of this committee from its

inception. During this time he has made or directed magnetic tests on possibly more compositions and in more conditions of heat treatment, than any other investigator. This statement is not made boastfully, but rather in humility, for in spite of the hundreds of compositions and the thousands of heat treatments tested he has heretofore published no results. They have not been withheld from selfish or trade motives but because, notwithstanding the vast amount of data collected, the question "What is magnetism?" remains unanswered, nor have any new theories or laws in regard to its manifestations been formulated. It has been felt, too, that the publication of wearisome tables recording mere test data would be of little interest or value, in view of the large number of such reports on record.

At the request of Dr. Charles W. Burrows and of Professor Marburg, however, the author submits certain of his results and observations, and feels warranted in so doing because at the Sixth Congress of the International Association for Testing Materials, no less than eight papers were submitted dealing with electrical and magnetic properties, and more especially because Professor Howe, in his scholarly presidential address at the meeting, referred to the desirability of developing tests which can be applied to finished articles without destroying them. It would be most useful if we could apply indestructive tests, such as electrical or magnetic, to various materials of construction and from the data obtained be able to conclude with certainty that certain other properties, mechanical or dynamic, are just what we desire. Professor Howe wisely suggests that these tests may be simply supplemental to our present tests to destruction. He says: "These indestructive methods have the defect of being indirect in one respect to weigh against their advantage of being direct in another; they are indirect in that they gage the properties actually needed in service by means of other properties; they are direct in that they may be applied to the very objects to be used, instead of vicariously to coupons or like objects to be destroyed in the test itself. Their natural service seems to supplement the vicarious destructive tests."

The Bureau of Standards has been conducting investigations along these lines and preliminary results have appeared, tending to show a relation between magnetic and physical char-

acteristics. The data herein presented may serve to amplify the work of others and to add something to our knowledge of this subject.

It will not be necessary to present an extended bibliography of the subject, for we are indebted to Prof. Silvanus P. Thompson for a recent one appended to his most instructive paper.¹

This article is an important addition to the literature of magnetism, and the bibliography appended thereto covers the classic investigations of every land. Nor should we omit reference to the symposium, international in character, upon magnetic properties of alloys, before the Faraday Society and presided over by Sir Robert Hadfield, F.R.S.²

APPARATUS USED.

In our investigations, which prior to 1908, were made in the research laboratories of the Sanderson Brothers Steel Co., and since that date at the Halcomb Steel Co., we have used the Esterline Permeameter. This instrument was devised and perfected by Prof. J. Walter Esterline, first chairman of the committee on magnetic testing. As it has been fully described,³ no further mention is required, but the reasons for using it and its advantages may be briefly stated.

1. It permits of testing materials of different cross-sections up to $1\frac{3}{4}$ by $\frac{3}{4}$ in.

2. It does not require the use of delicate instruments which are easily affected by vibrations, stray electric currents or magnetic fields: "orbs of virtue," as Dr. Gilbert styled them.

3. It is particularly suited to testing hard materials, and at high flux densities, to which these investigations have been principally confined.

4. Tests are made very rapidly, for in determining the magnetic characteristics of hard materials it is not necessary to determine the complete hysteresis loop, but only (a) the maximum induction for a given value of H which produces prac-

¹ "The Magnetism of Permanent Magnets," *Journal of Institution of Electrical Engineers*, Vol. 50, p. 217 (1913).

² *Transactions of the Faraday Society*, Vol. VIII, p. 94 (1912).

³ Report of Committee G on Magnetic Properties of Iron and Steel, *Proceedings, Am. Soc. Test. Mats.*, Vol. VI, p. 70 (1906); also Esterline, *ibid.*, p. 320.

tical saturation, or (b) to work at a constant maximum induction, noting the value of H which produces it, and under either method, (a) or (b), to determine also (c) the residual induction, and (d) the coercive force or field.

In our magnetic specifications we recommend method (b), and determine the residual induction and coercive force corresponding to a maximum induction of $B = 14,000$ gaussess.

In the author's work, method (a) has been employed for the most part, for two reasons. It was used many years before our specification was adopted and because $B = 14,000$ gaussess cannot be had in the case of many alloys, although it is readily obtained in the case of all alloys commercially used for permanent magnets.

MAGNETIC UNITS.

Let us review briefly the units and conventions used in magnetic testing and refresh our memories as to their significance. In the c. g. s. system we have the following:

1. The unit of magnetizing force, H , that is, the flux where iron is absent; we also speak of magnetizing force in terms of "Gilberts per centimeter."

2. When iron or other magnetic material is present the magnetic flux is greatly increased. This new flux is designated, B , and depends upon (a) the original value of H , and (b) upon the quality of the magnetic substance introduced into the field, H . B , therefore, is used to represent intensity of the flux or the maximum induction.

3. Permeability, represented by μ , is derived from B/H and is a variable depending upon the quality of the magnetic material and the field strength. That is, $B = \mu H$ and $\mu = B/H$.

4. When materials have been magnetized and the magnetizing field is reduced to zero, a certain percentage of the initial induction, B , is retained. This is called residual induction and is designated by the symbol, B_r .

5. Different substances retain magnetism with varying degrees of avidity or strength. The strength of field necessary to reduce B_r to zero, that is, to demagnetize the material, is called the coercive field or coercive force, and is designated H coer or H_c .

6. The unit of magnetic flux is called the gauss. We say, for example, a magnetizing force of $H=200$ gaussess produces in a given steel a maximum induction of $B=14,000$ gaussess.

NEW MAGNETIC UNIT PROPOSED.

For fully ten years the author has used a factor in connection with his work which he believes is of considerable value in judging of magnetic quality in hardened steels.

This factor is the ratio between residual induction, B_r , and coercive force, H_c , that is B_r/H_c . The coercive force alone is not a sufficient measure of permanence and just as we use μ to denote permeability, in other words the average number of lines of induced magnetism for each unit of magnetizing, H , so the factor B_r/H_c indicates the average number of lines of residual induction removed by one demagnetizing unit of coercive H . The smaller the numerical value of this quotient, the greater the absolute permanence of the residual magnetism. While by increasing the magnetic field, H , to very high values, the corresponding values of residual induction, B_r , and coercive force, H_c , gradually increase, yet the ratio B_r/H_c becomes almost constant above $H=200$ and therefore permits of comparing tests made at different values of H .

Table I shows that B_r/H_c becomes practically constant at initial inductions approaching saturation, that is, well beyond the "knee" of the magnetization curve.

It will be noted that the numerical values of B_r/H_c increase as the maximum induction increases. This corresponds to experience with permanent magnets. They seem to be super-saturated after first charging, and a portion of this residual magnetism is quite unstable. To render them fit for use it is customary to partially demagnetize them by maturing processes, and the "knocked-down" residual magnetism is then remarkably stable. We are not safe in inferring, however, that partially charging a magnet in the first place affords a better condition than supercharging, followed by partial demagnetization. The aging or maturing processes are necessary to effect certain molecular changes, and for the relieving of stresses which must be done to insure magnetic stability.

The new unit, B_r/H_c , is proposed for thoughtful considera-

tion of physicists and electrical engineers and it is hoped they will accept kindly the suggestion of a mere metallurgist. If experience shows the usefulness of this ratio, it will then be time enough to give it a name and to adopt a conventional designation to represent B_r/H_c . If used as a quality factor in connection with permanent magnet steel, it would be preferable to use the inverse ratio, H_c/B_r , since this would give increasing values for increasing permanence, but this would give decimal numbers, and therefore, to retain the advantage of increasing numerical values and to express these in whole numbers we have only to express B_r in kilogausses. For example, a given steel shows $B_r=12,000$ and $H_c=60$. Then $B_r/H_c=200$, as originally stated,

TABLE I.—PERMANENT MAGNET STEEL.

H	B	B_r	H_c	$\frac{B_r}{H_c}$
100	12 500	9 570	54	177.2
126	13 720	10 560	59	178.9
149	14 725	11 045	61	181.2
180	15 475	11 430	62	184.5
208	16 200	12 015	63	191.0
230	16 430	12 130	63	191.6
264	17 050	12 130	63	191.6

or $H_c/B_r=0.005$, using the inverse ratio, or 5.0, if instead of 12,000 gaussses we use 12 kilogausses. Another steel of the same residual induction and $H_c=72$, would give, $H_c/B_r=72/12=6$; that is, a higher quality factor because of higher permanence in proportion to the residual induction.

MAGNETIC VERSUS PHYSICAL HARDNESS.

The above sub-title is used for want of a better one. Magnetic hardness is not a definite physical constant like specific gravity or electrical resistance, nevertheless the term conveys a fairly definite idea. Physical hardness, too, cannot be definitely stated; we have mineralogical hardness, abrasive hardness, penetration hardness as measured by the Brinell ball test, or

elastic hardness as measured by the Shore scleroscope, and it is to the last two that we refer in the sub-title.

Broadly considered, there is a relation between magnetic and physical hardness, but is it a general one that can be stated definitely, or expressed by a formula or law?

At present this question must be answered in the negative. Magnetic hardness may be measured in terms of the decrease in maximum induction as compared to the softest pure iron, or we may measure it by the coercive force. Physical hardness at or near the surface, up to the limits of machinability and a little beyond, is best determined by the Brinell ball test. Great hardness in permanent magnets or hardened tool steel is difficult to measure. The scleroscope is most used on such materials, but its readings must be used with judgment and mostly for the comparison of similar materials and similar sizes.

Until quite recent years, it was generally believed that the purest iron was best for electromagnetic uses, and the softer it could be made, the better. This idea was rudely shattered when it was discovered that from 3 to 5 per cent of silicon added to mild steel, not only did not injure it magnetically but improved it electrically for dynamo and transformer use, yet the product was somewhat hardened by this addition of alloy. This was one of the surprises in connection with magnetic investigation to which reference has already been made.

In 1904, when the Brinell test was just coming into general use, the author hoped to find a definite relationship between magnetic hardness, as measured by H_c or B_r/H_c , and the Brinell number. At his suggestion, Prof. C. E. Freeman, of the Armour Institute of Technology, directed an investigation upon a variable carbon series of crucible steels furnished by the author. The results were privately communicated and showed that heat treatment of a given steel affects both its physical and magnetic hardness, but not always in the same ratio, whether tested with constant H , or with constant B . It was found that for an ascending carbon series, with the same heat treatment, there was in some cases a definite relation between coercive force and Brinell hardness. In fact, for two conditions of treatment, when H_c was plotted against the Brinell number a straight-line relationship was found, showing the influence of carbon on

both properties. In other conditions of heat treatment no semblance of a curve could be plotted. It is expected to repeat and verify these results and to amplify them before presenting the actual results obtained, for it is felt that the tests were not made at sufficiently high induction values.

EFFECT OF SIZE UPON MAGNETIC PROPERTIES.

It has long been known that the greater the ratio of length to cross-section, the greater the permanence of the residual magnetism in a bar magnet. The author does not recall any experiments where varying diameters have been tested, keeping the length proportioned to the diameter. In using the Esterline apparatus we have an almost closed magnetic circuit of small and constant reluctance, yet pieces of widely varying section

TABLE II.—EFFECT OF CROSS-SECTION UPON MAGNETIC PROPERTIES.
MAGNET STEEL, WATER HARDENED.

Area, sq. in.	H	B	B_r	H_c	$\frac{B_r}{H_c}$
0.798	210	14 545	10 115	71.50	141.4
0.485	208	15 515	10 890	72.75	149.7
0.325	210	15 735	11 315	70.50	160.8

Analysis: Carbon, 0.65 per cent; tungsten, 5.22 per cent; manganese, 0.33 per cent.
Treatment: Quenched in cold water from 900° C. (1650° F.).

may be tested. It occurred to us to take the product of a single ingot and roll it into bars of different cross-sections. Pieces of different sections were then hardened in water and subjected to test. The results were directly contrary to what was expected for the induction B , and residual induction B_r , increased as the size decreased, while B_r/H_c showed the largest section to be most permanent. This is contrary to experience in the making of horseshoe magnets, but it must be recalled that ordinarily for any given purpose, only the cross-section in them is changed, while the length and air-gap remain constant. Hence the rule applying to bar magnets applies to some extent to them, and as the ratio of section to length increases, the demagnetizing effect of the poles increases and a weaker finished magnet results from the use of large sections.

In Table II the results of these tests are given.

It may be argued that this may be due to some error in the instrument or its calibration, but the results of tests on a new set of bars made from the same ingot were equally conclusive, and in two other series, hardened in oil, the opposite result was found, namely, the smallest section gave the best test, and in only the very smallest bars was the rate of quenching in oil fast enough to develop high permanence. It seems, therefore, that the differences cannot be due to the apparatus but to the reactions of quenching. The author is unable to explain this magnetic behavior upon any present theory of hardening or of the constitution of steel.

In Table III the results for oil-quenched specimens are

TABLE III.—EFFECT OF CROSS-SECTION UPON MAGNETIC PROPERTIES.
MAGNET STEEL, OIL HARDENED.

Area, sq. in.	H	B	Br	H_c	$\frac{Br}{H_c}$
0.781	206	18 600	11 935	37	323.0
0.469	203	17 825	11 700	40	292.5
0.317	206	16 275	10 850	45	241.0
0.156	204	14 260	9 920	70	141.7

Analysis: As in Table II.

Treatment: Quenched in lard oil from 875° C. (1600° F.).

given, and a comparison of Tables II and III will prove most interesting. It might be added that each different bar used in both tables was analyzed completely for identification.

The above bars were all approximately $1\frac{1}{4}$ in. wide, with variable thickness. Another series of bars $\frac{5}{8}$ in. wide showed similar results, that is, the lightest section shows the greatest permanence.

If there remains any doubt that the effect of mass is due to inherent constitutional differences in the steel itself, it may be dispelled by citing another steel of altogether different type of analysis, in which both for water and oil hardening the large section shows the best test for permanence. The steel in question was of the following analysis: carbon, 0.61 per cent; silicon, 0.54 per cent; manganese, 0.82 per cent; chromium, 0.80 per

cent. This steel was tested in 1 by $\frac{1}{4}$ -in., and 1 by $\frac{3}{4}$ -in. sections. The results are shown in Table IV, the first two specimens being the water-hardened samples and the last two the oil-hardened samples.

Judging either by the coercive force, H_c , or by B_r/H_c , it is noticed that the larger section is best for permanence. There is also disclosed a new and important observation, which will be discussed in the next section of the paper.

STEELS HARDENED IN OIL.

Considering the case of an individual steel, capable of hardening, it has been thought that the harder it could be made

TABLE IV.—EFFECT OF CROSS-SECTION UPON MAGNETIC PROPERTIES.
CHROME STEEL, WATER AND OIL-HARDENED.

Area, sq. in.	H	B	B_r	H_c	$\frac{B_r}{H_c}$
0.250	208	15 070	9920	48.0	206.6
0.750	209	14 895	9840	50.5	194.8
0.250	208	14 025	9375	50.5	185.7
0.750	208	14 260	9100	55.0	165.5

Analysis: Carbon, 0.61 per cent; silicon, 0.54 per cent; manganese, 0.82 per cent; chromium, 0.80 per cent.

Treatment: First two quenched in water from 843° C. (1550° F.); second two in oil from same temperature.

the greater its permanence. Brine and even mercury have been resorted to in quenching steels, with the idea that they would hasten the rate of cooling, impart greater hardness, and therefore put the metal in best condition to retain magnetism. Comparative tests in such cases would be expected to show that the harder the piece, the less permeable, the lower its residual induction and the higher its coercive force. This is the prevailing idea, without question, but it is only true with certain steels and under restricted conditions.

It is well known that the hardening capacity of steel decreases as its mass increases, yet in the previous section we have shown that the larger mass and hence the softer piece is magnetically most permanent.

It is well known, too, that the rate of cooling in oil is slower than in water and does not give as great hardness, yet in Table IV we see the relatively soft oil-hardened piece showing for equal sizes, lower induction, lower residual and higher coercive force than the much harder water-quenched piece, and the factor B_r/H_c shows the superior permanence of the oil-quenched steel very clearly.

Table V shows the average of six different bars from the same ingot quenched in both oil and water. The Brinell and scleroscope figures are also given.

In Table V, charge No. 1 was hardened in cold water, No. 2 in oil, and No. 3 in boiling water, and in all its properties No. 3 will be found to lie between Nos. 1 and 2.

TABLE V.—OIL HARDENING VS. WATER HARDENING AND THE EFFECT UPON MAGNETIC PROPERTIES AND PHYSICAL HARDNESS.

$H=208$

Charge No.	Scleroscope Number.	Brinell Number.	B	B_r	H_c	$\frac{B_r}{H_c}$
1	73.5	594	16 700	11 415	44.60	256.0
2	63.0	534	14 330	9 040	52.40	172.5
3	70.0	548	15 500	10 285	50.00	205.7

Analysis: Carbon, 0.50 per cent; silicon, 0.51 per cent; manganese, 0.79 per cent; chromium, 0.73 per cent.

Treatment: (1) Average quenching temperature of six bars, 815° C. (1500° F.), in cold water.

(2) Average temperature of six bars, 843° C. (1550° F.) in oil.

(3) Average temperature of two bars, 843° C. (1550° F.) in boiling water.

The results were verified in three separate series of tests on variable carbon ingots of the same type of steel, whose analysis was given above. The results in every case were of the same order as shown in Table V. It may be asked: Is this a peculiarity of this particular type of steel? The answer is: No, it is not. It seems to be generally true of the great class of structural alloy steels, such as are covered in our automobile-steel specifications. The author has tested it with silico-manganese steels of 0.47 to 0.68 per cent of carbon. It holds good for chrome-nickel, 3.5 per cent of nickel, and nickel-vanadium steels of various carbons and compositions that we have tested. Strange to say it does not hold good for the two types

of steel most used for permanent magnets, namely, common open-hearth magnet, of the type represented by carbon 0.60 per cent, and manganese 0.75 to 1.00 per cent, and high-grade tungsten magnet steel of the type, carbon 0.60 per cent and tungsten 5.00 per cent. Moreover, the two elements which primarily confer the property of oil hardening, manganese and chromium, may be added to the tungsten steel in fairly large quantities and still not change it from a steel whose best properties are developed by water hardening.

DRAWING THE TEMPER AND THE EFFECT ON MAGNETIC PROPERTIES.

In the previous section it was shown that retarded rates of cooling either by oil quenching or hot-water quenching, lessen the ultimate hardness of the piece quenched, and in many steels this results in decreased maximum induction and residual magnetism and increased permanence.

Now hardness may be relieved also by drawing the temper, but this gives results directly contrary to those due to retarded quenching rates. That is, the maximum induction and residual magnetism are increased and the coercive lowered. This is true whether the steel owes its original hardness to oil or water hardening, and it is also true that the effect of tempering is more marked on oil-hardened than on water-hardened pieces. That is, the augmented permanence in these alloy steels, due to oil hardening, is unstable, and this fact limits the possible commercial application of oil hardening of magnets in manufacture.

Boiling in water has but slight effect upon the magnetic tests, while drawing the temper at 205° C. (400° F.) seriously impairs the magnetic permanence, and this is true of both the structural alloys and the commercial magnet steels. At a drawing temperature of 315° C. (600° F.), the difference between oil and water tempering is wholly wiped out and nearly identical tests result. Table VI shows that drawing to 315° C. has softened the oil-hardened pieces physically to a less degree than the water-hardened piece. On the contrary, the greatest magnetic changes are in the oil-hardened steels.

While drawing to 315° C. has lowered the Brinell hardness

101 points in the water-hardened pieces, it has lowered the oil-hardened pieces but 36 points; on the other hand, the change in B , B_r and H_c is much greater in the oil-hardened bars than in the water-hardened ones. Yet the total change has been to bring both physical and magnetic properties approximately to a level and to wipe out the marked differences in the original properties, due to oil and water hardening.

TABLE VI.—EFFECT OF DRAWING THE TEMPER UPON PHYSICAL AND MAGNETIC HARDNESS.

$H=210$.

Heat Treatment.	Brinell Number.	B	B_r	H_c	$\frac{B_r}{H_c}$
Water quenched.....	629	16 275	11 085	46	241
Oil quenched	546	13 485	8 550	52	164
Water quenched and tempered...	528	19 995	12 960	22	589
Oil quenched and tempered.....	510	19 315	12 570	22	571

Analysis: Average of duplicate tests on each of three different chromium steels of type shown in Table V. Steels differed but slightly from type analysis.

Treatment: Six quenched in cold water from 843°C . (1550°F .); six quenched in oil from 843°C .; after testing, all were drawn in oil bath for $\frac{1}{2}$ hour at 315°C . (600°F .).

A specific case rather than average results is even more interesting. Steel from the same ingot and bar, hardened in oil, shows a scleroscope number of 71, and a Brinell number of 570, and hardened in water and drawn to 315°C . shows a scleroscope number of 70, and a Brinell number of 567. Certainly, a close agreement as we measure hardness, yet what a difference in magnetic properties! Let us compare them in Table VII.

TABLE VII.—COMPARISON OF OIL-QUENCHED AND WATER-QUENCHED STEELS.

Treatment.	B	B_r	H_c	$\frac{B_r}{H_c}$
Oil, 843°C	13 060	8 695	52.5	166
Water, 843°C ., drawn at 315°C .	19 600	12 865	23.0	472

What molecular changes account for the difference? What constitutional changes varying the proportions of alpha, beta and gamma iron, or martensite, sorbite and troostite? The

author hopes the theoretical side of the subject will appeal to those better able to produce an explanation.

It was stated above that these results are not characteristic of one particular steel. In Table VIII are given results confirming this.

TABLE VIII.—MISCELLANEOUS ALLOYS.

Group No.	Magnetic Test.					Heat Treatment.
	<i>H</i>	<i>B</i>	<i>B_r</i>	<i>H_c</i>	$\frac{B_r}{H_c}$	
1	206	18 600	12 400	17.0	729	Annealed, 760° C.
	206	16 740	10 400	40.0	260	Oil, 816° C.
	206	17 825	10 850	36.0	301	Water, 816° C.
	206	20 075	11 160	23.0	485	Water or oil, 816° C.; drawn 315° C.
2	202	20 150	12 400	10.0	1 240	Annealed, 816° C.
	202	17 515	10 230	33.0	310	Oil, 816° C.
	202	19 975	10 700	28.0	382	Water, 816° C.
	202	20 900	11 060	18.0	615	Water or oil, 816° C.; drawn 315° C.
3	202	19 750	12 710	10.5	1 210	Annealed, 816° C.
	202	16 900	10 400	35.5	293	Oil, 843° C.
	202	17 515	10 630	24.5	434	Water, 843° C.
	202	20 770	12 400	18.0	688	Water or oil, 843° C.; drawn 315° C.
4	206	18 600	13 640	10.0	1 364	Annealed, 816° C.
	206	13 330	8 060	47.0	171	Oil, 945° C.
	206	16 275	10 850	44.5	244	Water, 945° C.
	206	19 530	15 500	19.0	816	Water or oil, 945° C.; drawn 538° C.
5	206	17 670	8 525	24.0	355	Oil, 870° C.
	206	14 453	8 725	57.0	153	Water, 870° C.
6	208	13 950	9 450	39.0	242	Oil, 870° C.
	208	15 810	11 935	57.0	200	Water, 870° C.

Analysis (per cent): Group No. 1: carbon, 0.24; silicon, 0.21; manganese, 0.46; chromium, 0.96; nickel, 2.02.

Group No. 2: carbon, 0.25; silicon, 0.21; manganese, 0.74; nickel, 3.55.

Group No. 3: carbon, 0.30; silicon, 0.19; manganese, 0.64; nickel, 3.25; vanadium, 0.18.

Group No. 4: carbon, 0.47; silicon, 1.83; manganese, 0.70.

Group No. 5: carbon, 0.54; silicon, 0.11; manganese, 0.78.

Group No. 6: carbon, 0.59; silicon, 0.28; manganese, 0.49; chromium, 0.23; wolfram, 5.30.

Four typical structural alloy steels are shown in the annealed, oil-hardened, water-hardened, and tempered conditions. In comparing the oil and water-hardening results of the first four steels, the former are seen to give the greater magnetic hardness in every case as measured by B_r/H_c . In regard to the results for hardened steels, drawn to 315° C., or in one case to 538° C., the drawing has reduced both the oil and water-hardened specimens to values so nearly identical that the author has disregarded the slight differences, which for B and B_r did not exceed 2 per cent, and for H_c were not more than one unit apart. In the case of silico-manganese steel only, was 315° C. insufficient to "level" the results, but 538° C. produced this leveling effect.

Another interesting observation in connection with annealed versus hardened and tempered specimens is that B is always higher in the latter condition, but B_r/H_c always shows the annealed alloys to be magnetically softer than the hardened and tempered steels.

The last two steels illustrate the fact that the two types of commercial, permanent magnet steels do not behave like the other alloys, in that their best condition for permanence is developed by water hardening.

In all the analyses given sulfur and phosphorus have been omitted. Nearly all the products used in our experiments, excepting the carbon-manganese magnet steel, were produced in our electric furnace, and it may be assumed that they do not exceed 0.015 per cent. This chemical purity is quite characteristic of our alloy products and we may be permitted to mention that such steels were first made in America by the Halcomb Steel Co. Sulfur and phosphorus below 0.01 per cent is frequent, and either sulfur or phosphorus above 0.02 per cent is almost never seen.

CONCLUSION.

Have we not maintained our thesis that the fascination of magnetic investigations lies in the frequency of unexpected results? But unexpected results are not necessarily incapable of explanation. They are unexpected because we do not know the "rules of the game," and because our mental conceptions

of the relationship between physical properties and magnetic properties are in error. There must be laws governing both. Can we discover them? Evidently our fundamental information in regard to the magnetic habits of steel and alloys is insufficient. Who will supply the missing knowledge? The author hoped to do so, unaided, but now recognizes the immensity of the task and contributes this mite in the hope that others will do likewise and that further investigations will be stimulated by the results herein recorded.

Many have aided in this work since its inception, but particularly the author wishes to thank Mr. Howard J. Stagg, Jr., metallurgist of the Halcomb Steel Co., for his conscientious work in conducting the experimental work on heat treatments and magnetic tests, a very small fraction of which is embodied above. His unfailing interest and assistance have made this paper possible.

The writer is also indebted to Dr. Charles W. Burrows for reviewing this paper and for valuable suggestions in regard to it.

DISCUSSION.

MR. C. W. BURROWS.—Even though time is at a premium, Mr. Burrows, this paper contains so much that is new and suggestive that it should not be passed over without comment.

For many years it has been known that every change that a bar of steel may undergo, brings with it a corresponding change in its magnetic behavior. This rule of interrelation has no known exceptions. These relations are at present known only approximately, and when fully known will probably prove to be more or less complicated. The reason for this complication is easily understood when we consider that the magnetic nature of a bar of steel requires for its complete identification not one quantity but many.

Some of the more important characteristics may be mentioned:

1. The initial permeability designated as μ_0 was used in the magnetic hardness testing apparatus of Schuchardt and Schütte.

2. The maximum permeability indicates an important characteristic of the material.

3. The residual induction which remains in a piece of steel after it has been magnetized is an important criterion both of mechanical and magnetic nature. Barus and Strouhal gave considerable attention to the relation between this magnetic characteristic and certain mechanical characteristics.

4. The energy required to carry the sample through a complete magnetic cycle is known as the hysteresis loss. The relation between this magnitude and certain other physical and chemical characteristics has been discussed before this Society in a paper by C. W. Waggoner.

5. The coercive force is the magnetic field necessary to reduce the magnetization of the specimen to zero. This constant is an important criterion of the physical nature of the steel. However, but little has been published on it.

Mr. Burrows. Mr. Mathews uses for his criterion of the magnetic nature of steel, the value of the ratio B_r/H_c and finds this quantity a valuable criterion of the fitness of the steel for use in permanent magnets.

Some one in Australia, whose name I do not at present recall, has also made practical use of the magnetic permeability, to determine when a mine-elevator cable becomes unfit for use through either excessive strain or breakage.

While it has been shown that for a given purpose, the relative merits of different steels which do not differ too widely in their chemical composition and heat treatment, may be determined from a single magnetic characteristic, it is too much to expect that any single variable will define all the physical characteristics of any steel. Such an expectation would be as unreasonable as to expect the complete behavior of a steel to be defined in terms of its carbon content irrespective of its other constituents, or to be defined in terms of its Brinell numeral irrespective of its heat treatment.

In view of the fact that every chemical thermal or mechanical change in a steel brings with it a change in the magnetic characteristics and that this relation is reciprocal, it is evident that there is one and only one physical condition corresponding to a given set of magnetic characteristics. Granting this, the problem that confronts the investigator along this line, is to correlate the magnetic and other characteristics of steel and then pick out the minimum number of magnetic variables which will indicate with sufficient accuracy the other physical properties desired.

In the present paper Mr. Mathews has made a valuable contribution to this subject. My experience while it is not so extensive as that of Mr. Mathews is entirely corroborative. However, I believe that it is quite possible that a different magnetic criterion will in some cases give a better indication of the mechanical properties.

In Tables II and III our attention is called to the fact that the quotient B_r/H_c shows increasing values as the cross-section diminishes for water quenching, and the reverse for oil quenching. If, however, we take as our criterion the product $B_r \times H_c$ we find that both for water and for oil quenching, the magnetic hardness

is greater for the small cross-sections. This removes the anomaly **Mr. Burrows.** which so bothered the author of the paper.

If we use this same product as the criterion for the material of Table IV we find that the two methods of quenching give the same magnetic numeral. This is true for the small section as well as for the larger section.

This same criterion applied to Table VI will arrange the four specimens in the same order as their Brinell numerals.

Table VII in the form in which it stands furnishes some remarkable data. We have here two bars of the same material which have been submitted to different heat treatments. Every one is willing to admit that these treated steels have different mechanical properties. Yet, by the scleroscope and Brinell tests they are identical. On the other hand, all four pairs of the magnetic constants agree in indicating a difference in material.

Viewed from this angle I think the magnetic data of this paper are, at least qualitatively, a criterion of the character of the steel.

Mr. Mathews has found the quotient B_r/H_c to be a valuable criterion of the fitness of steel for use in permanent magnets and proposes it as a new magnetic unit. I agree with him that this quotient is an important index for magnetic steel. However, I prefer to use his alternative of the reciprocal ($= H_c/B_r$). This has the advantage of giving the larger numerical values to the better magnetic steels. As a suitable name, I suggest the word "coercivity."

MR. R. P. DEVRIES.—Mr. Burrows suggests that for **Mr. Devries.** certain tables given in the paper the product $B_r \times H_c$ should be used in place of the ratio B_r/H_c . I should presume that Mr. Mathews has chosen the ratio because it fitted in best with all of the data he has obtained, and that some of the published tables were given to show that this magnetic criterion, like others chosen in the past, is not always in good agreement with the results of hardness or other mechanical tests.

It is to be regretted that for practical purposes we are confined to one magnetic criterion, whereas the complete magnetic curve offers an infinite number of criteria.

MR. J. A. MATHEWS.—The use of the product as suggested **Mr. Mathews.** by Mr. Burrows is entirely new to me. I can not say whether this

Mr. Mathews. product would be erratic or whether it would always agree with the physical characteristics. This paper was brought about in part by some remarks made in Mr. Howe's Presidential Address at the last International Congress, in regard to the use of destructive tests. I have been engaged in the making of magnetic tests in conjunction with other tests on alloy steels a great many years, so I thought this might prove a start for an accumulation of data that might ultimately be of value, as Mr. Howe has suggested. We must have an accumulation of data or, we will have hard work to say what we may expect physically from magnetic tests, but the task is not hopeless. I am not undertaking to explain the results embodied in my paper on any theoretical ground; I leave that to Mr. Howe and others who are better informed on the theory of the subject than I am.

Mr. Flowers. **MR. ALAN E. FLOWERS.**—A question that naturally occurs in comparing hardness and magnetic tests of this kind is that the Brinell and scleroscope instruments test the surface condition, while the magnetic tests may or may not test the surface condition, depending upon the rapidity of the magnetic change. The interior of the piece may be different in its hardness characteristics from the surface, and if the magnetic test is made slowly, the condition of the interior will determine to a considerable extent the amount of magnetic effect obtained—in other words, the value of the magnetic density obtained for any given magnetizing force. If, on the other hand, the magnetic test is made with apparatus working at rather high frequency, the magnetic flux may penetrate the interior of the specimen to only a very small degree, and consequently the amount of flux obtained will be a function of the frequency of the alternating current applied or of the rapidity of change, if it is a single change. These two things, it seems to me, deserve very serious consideration in any magnetic test which is to be compared with physical tests made upon the surface.

I should like to ask the author as to the value of H for the test reported in Table VII. The values are given, I believe, in the other tables, but I happen not to have found it in this table.

Mr. Mathews. **MR. MATHEWS.**— H was practically constant at 200 to 210, in that particular table.

MR. FLOWERS.—I should like to point out that the magnetic conditions are not by any means constant over a cross-section. The permeability will be different in the direction of rolling from that in the direction at right angles to the direction of rolling, and different along the edge of the specimen from that near the center. Some years ago we conducted some tests in which we had a rectangular flux path built up of ordinary transformer iron, where the length of the path around the shortest distance was less than half of the length of the path around the greatest perimeter; even allowing for cut edges or even assuming a circular path at the corners. Under those conditions one might expect that the value of the flux density would be very markedly less in the outer section than in the inner section. No such thing occurred. The value of the flux density was slightly less in the middle of the sheet. But at the outer edge, the flux density was practically the same as that in the inner fourth of the section. This is partly due to the fact that the flux passing down the straight path tends to spread itself over the section, and partly to the variation of permeability with flux density. Mr. Flowers.

HARDNESS TESTS: RELATION BETWEEN BRINELL BALL-TEST AND SCLEROSCOPE READINGS.

BY J. J. THOMAS.

A number of inquiries have recently reached Watertown Arsenal as to the proper factor to convert Brinell ball-test hardness numbers to scleroscope readings. Many manufacturers and laboratories have one of these instruments, but few have both. In order, therefore, to compare the hardness of their product with that of another factory or to interpret the results given in various papers and journals, it is necessary that the factor connecting the two be known.

The curves shown in Fig. 1 were plotted after taking over 500 readings. All Brinell impressions were made with an "Alpha" machine having a steel ball 10 mm. in diameter, with a pressure of 3000 kg. for 30 seconds. The scleroscope readings were taken with a Shore instrument, having a diamond-tipped hammer.

Different metals have been given different symbols in order that the relative hardness may be quickly noted. The full line gives the relation for steels, and is believed to be a representative curve. For this line the factor is 6.67; that is, the scleroscope reading multiplied by 6.67 gives the Brinell-ball hardness numbers.

It was found on plotting the values for cast iron and bronzes that all points were above this line. The broken line shown was therefore plotted, giving a factor of 5.25 for these metals. The three points for aluminum lie between these two lines, giving a factor of about 6. The points for nickel steel lie below the full line, and for these the factor 7.7 seems more suitable.

From the above it is seen that the factor is not constant, but varies with the different metals. In fact, considerable variation is found even in the same metal. The scleroscope readings vary more than the ball tests, probably due to the fact that the

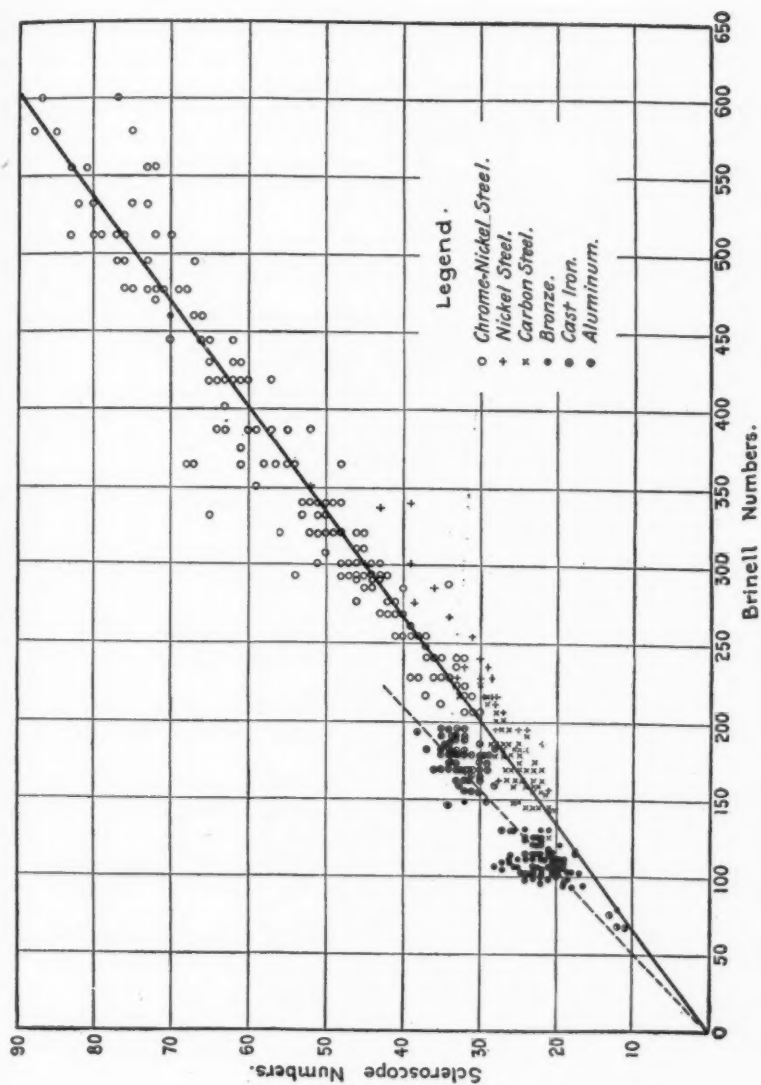


FIG. 1.—Comparison between Brinell-Ball and Shore-Scleroscope Hardness Numbers.

scleroscope measures the hardness of very small areas, and these areas vary in hardness even in the same metal.

It has been found that for hardness numbers above 300 Brinell or 45 scleroscope, the metals are very difficult to machine. Tempered steels lie in the range of 150 to 300 Brinell.

No attempt has been made to compare the accuracy or usefulness of the two instruments, as it is believed that each has a place in the testing of materials.

DISCUSSION.

MR. W. C. CUSHING (*by letter*).—For some years, Brinell- Mr. Cushing.
ball and scleroscope tests have been made on Bessemer and open-
hearth rail steel at the Altoona laboratory of the Pennsylvania
Railroad Co. The average ratio between the Brinell and
scleroscope readings, from 1000 tests recently made, is 7.56,
which factor is quite different from that for carbon steel given
by Mr. Thomas.

The extent to which this relationship varies is indicated by
the minimum of 4.97 and the maximum of 14.11. The range
of this ratio was found to be as follows:

Ratio between		4 and	5 was found in	0.2 per cent of the tests.	
"	"	5 "	6 "	"	" 5.0 "
"	"	6 "	7 "	"	" 20.4 "
"	"	7 "	8 "	"	" 47.3 "
"	"	8 "	9 "	"	" 23.2 "
"	"	9 "	10 "	"	" 2.2 "
"	"	10 "	11 "	"	" 0.6 "
"	"	11 "	12 "	"	" 0.5 "
"	"	12 "	13 "	"	" 0.3 "
"	"	13 "	14 "	"	" 0.2 "
"	"	14 "	15 "	"	" 0.1 "

MR. J. J. THOMAS (*Author's closure, by letter*).—The readings Mr. Thomas.
made by me on carbon steels give a value for the ratio not
greater than 7, as shown by the curve. As Mr. Cushing's factor
is based on the results of 1000 tests, it is probably nearer correct
than mine, as my points were plotted from the results of about
50 tests.

Attention is called to the fact that many of the points
plotted represent a number of similar values, especially those
points which lie close to the curve.

No such variation was found as that reported by Mr. Cush-
ing. It is believed that the manner of holding the specimen
for the scleroscope readings has considerable effect. Small
pieces should be held in a vise.

A FAILED AXLE: STUDY OF AN INTERNAL TRANSVERSE FISSURE.

BY ROBERT JOB.

SUMMARY.

The paper gives the results of the investigation of a 10-in. driving axle just received from the manufacturers and which had broken in two while a cut was being removed prior to mounting the axle for service.

An internal transverse fissure, and also internal longitudinal fissures, were found upon the fractured surfaces, and the investigation proves that the defects which caused the failure of the axle occurred in the mill during manufacture, partly as a result of unsound steel, and partly owing to intense strains induced by unequal cooling of the steel in or after removal from the annealing furnace.

The investigation as a whole is of significance since it proves that internal transverse and longitudinal fissures have developed in medium carbon (0.48 per cent) steel in the mill as a result of defective manufacture. The paper is illustrated by eight photomicrographs and photographs.

A FAILED AXLE: STUDY OF AN INTERNAL TRANSVERSE FISSURE.

BY ROBERT JOB.

The type of failure known as "internal transverse fissure" has been carefully studied, particularly in connection with rails which have failed in service, and much evidence has been collected as to the prime cause or causes of the defect, as well as to the method of its development.

Dormus in his studies of these fissures about fifteen years ago, upon Austrian low-carbon, Bessemer rails—0.20 to 0.40 per cent carbon—reached the conclusion that they were produced either in the mill itself, or subsequently in service, but only in unsound steel. In this country these failures have been found heretofore, as far as we are aware, after the steel has been in service.

In view of the importance and the dangerous character of this type of failure, we have taken especial interest in the study of a 10-in. driving axle which recently came under our observation. The axle in question was of plain carbon steel, annealed, and said to have had no subsequent heat treatment. It was received in a shipment direct from the manufacturers and had never been in service. Upon receipt at the shops, while a cut was being removed in a lathe prior to mounting, the axle broke in two at a point about 20 in. from the end. The appearance of the fracture was peculiar, showing a band of bright, clean, unoxidized metal around the circumference of the axle and extending about 1 in. toward the center. Inside of this band the surface of the metal was discolored and oxidized. This oxidized condition did not extend into the steel longitudinally, excepting as mentioned below, but was simply an oxidized transverse crack or fissure which extended over the inner portion of the fractured area.

The surface of the transverse crack was rough and irregular, and two longitudinal fissures were found, one across the center of the axle and the other about $1\frac{1}{2}$ in. from the circumference.

The appearance of the fractured surface is shown in Fig. 1, the six round holes indicating the locations from which borings were taken for analysis, from the inside and from the outside portions, respectively.

The analyses are as follows:

	OUTSIDE.	INSIDE.
Carbon, per cent.....	0.48	0.47
Phosphorus, per cent.....	0.022	0.019
Manganese, per cent.....	0.46	0.46
Sulfur, per cent.....	0.036	0.031
Silicon, per cent.....	0.142	0.161

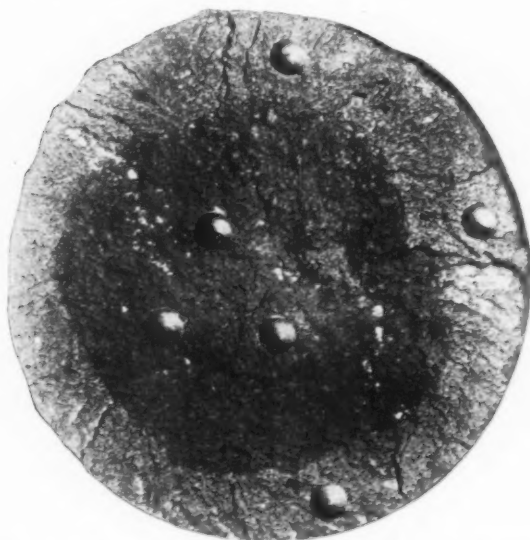


FIG. 1.—Fractured Surface, showing Oxidized Internal Transverse Fissure with Bright Metal around the Outside Portion.

The chemical composition, outside and inside, is closely alike and is within the usual limits, and does not indicate the cause of failure, and the fact that the proportion of silicon is moderate indicates that no excessive amount of slag was present, although slag inclusions were found.

Test specimens were cut longitudinally from the axle, one from the bright outside portion close to the surface, and one

from the oxidized portion about half way between center and circumference. The results of the tension tests are as follows:

	OUTSIDE.	INSIDE.
Tensile Strength, lb. per sq. in.....	92,340	76,030
Elastic Limit, lb. per sq. in.....	54,080	45,410
Elongation in 2 in., per cent.....	14.5	5.5
Reduction of Area, per cent.....	29.6	12.3

In the outside portion it will be noted that the elongation and the reduction of area of the metal are below normal, while in the inside portion the results indicate radically defective material.

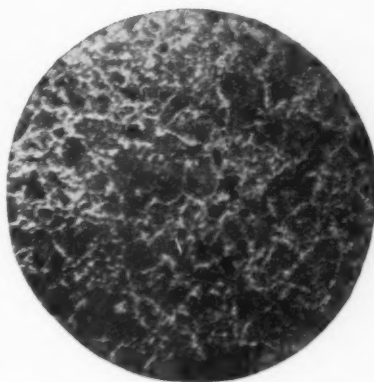


FIG. 2.—Bright Outside Portion about $\frac{1}{2}$ in. from Surface, Polished and Etched with Iodine; ($\times 50$).



FIG. 3.—Inside Oxidized Portion, Midway between Center and Circumference, Polished and Etched with Iodine; ($\times 50$).

Microscopic examination was made from sections cut from the above test specimens with results shown in Figs. 2 and 3, at 50 diameters. From these photomicrographs it will be noted that the size of grain in the outside portion of the axle was coarser than that in the inside portion, and that the size in the inside portion was fairly fine, indicating a proper annealing temperature at that point. A slag inclusion is shown in each of the photomicrographs.

We next cut a transverse section from the axle close to the point of fracture and on polishing and etching with iodine we obtained the result shown in Fig. 4. In this we found that one side of the axle—the upper part in the figure—was coarse-grained, and a photograph at 2 diameters of this upper portion is shown in Fig. 5. Fig. 6 is a photograph at 2 diameters taken at the center of the axle, and shows the line of one of the longitudinal fissures, and the fine-grained structure, while Fig. 7, also at 2 diameters, shows the condition in the lower part of



FIG. 4.—Transverse Section, 1 in. from Fracture,
Polished and Etched with Iodine.

Fig. 4 at the side opposite Fig. 5 and at about the same distance from the circumference as the latter. The granular size is fairly fine, and the gradual coarsening near the surface of the axle is shown.

The steel throughout the section was rather porous and contained occasional small cavities and slag inclusions. Fig. 8, a photograph at 50 diameters, represents the condition at one of these locations.

Upon study of the sections shown above, several facts are indicated. In the first place the relatively coarser grain upon

the outside surface around the entire circumference than inside proves that the temperature of the annealing furnace was higher than necessary to produce the best results, and in consequence, by the time that the refining temperature had penetrated to the center of the axle, obliterating the original coarse granular form, the steel upon the outside portion had been overheated, causing increased granular size, and resulting in a lower elongation and reduction of area than would have occurred otherwise. This

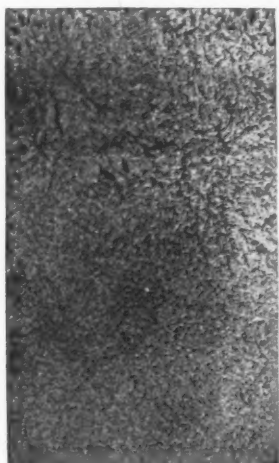


FIG. 5.—Upper Outside Portion of Axle (Fig. 4) showing Coarse Grain near the Surface; ($\times 2$).



FIG. 6.—Center of Axle (Fig. 4) Fine Grain and Longitudinal Fissure; ($\times 2$).

condition, therefore, simply means that the annealing temperature was not properly controlled; in other words, it is evidence of lack of careful mill practice.

Secondly, the fact that the size of grain is decidedly larger upon one side of the axle than upon the other, proves that the temperature upon the one side was decidedly higher than that upon the other. This condition could be produced by use of an unevenly heated annealing furnace, or it would also result provided the axles in the charge after annealing had been cooled

unequally in any manner, as, for example, by opening the door of the furnace in the winter, and exposing the adjacent steel to the cold air, thus chilling the side exposed to the cold, while the temperature of the other side was kept up by the other red-hot axles, and by the heat of the furnace. The same condition could also occur if a charge of axles was removed from the annealing furnace when at or above a red heat, and allowed to remain piled together in some location where the outside portion was exposed to cold. The effect would be to chill the side of



FIG. 7.—Lower Portion of Axle near Surface (Fig. 4), showing Fine Grain except near Surface.

an axle exposed to water or to cold air, while the other side of the axle in contact with or adjacent to other red-hot axles might easily be maintained for a considerable time at a temperature above the critical point of the steel. Under these conditions a decided variation in the size of the grain upon the opposite sides of the axles so exposed could easily result, and owing to the difference in relative rates of contraction, severe torsional strains would be induced which might easily cause rupture of the metal and produce a transverse crack such as is shown in

Fig. 1, as well as the longitudinal cracks. The fact that the surface of the transverse fissure was oxidized is easily explained by the presence of the longitudinal cracks, one or both of which evidently extended to the surface of the axle and thus admitted air or moisture to the heated surface of the fissure.

The fact that the steel was porous in spots, and contained slag inclusions, is merely an indication of insufficient cropping to get to sound metal—in other words, defective mill practice; and such condition would naturally result in weakness in the

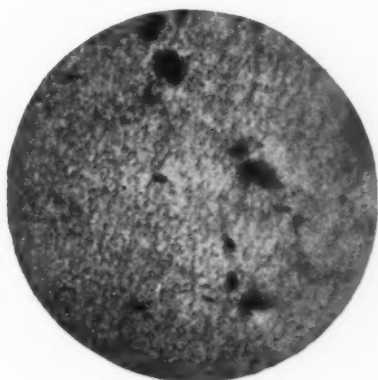


FIG. 8.—Porous Steel; ($\times 50$).

steel and would thus lessen the force necessary to rupture the metal and to produce fissures.

The relative weakness and brittleness of the interior portion as shown by the tensile properties is fully accounted for partly by the evident lack of proper cropping at the mill and partly by the intense strains to which the metal had been subjected.

The study taken as a whole is of interest, since it proves that an internal transverse fissure may be produced in metal in the mill and as a result of severe stresses induced by defective mill practice.

In conclusion, the assistance of Mr. F. F. White, in connection with some of the photographic and microscopic work, is acknowledged.

DISCUSSION.

The Chairman. **THE CHAIRMAN (MR. ROBERT W. HUNT).**—You have Mr. Job's paper before you, gentlemen, and as transverse fissures are a matter of such live interest, anything bearing upon this subject is of great value. Personally I am quite convinced that the mill is the place where they originate and that it is a mistake to suppose that the subsequent service is responsible for their presence or rather their inception. It would seem that Mr. Job's conclusions rather agree with my own feelings in the matter. The paper is open for discussion.

Mr. Campbell. **MR. WILLIAM CAMPBELL.**—Mr. Job's paper brings out a comparatively little known or seldom-met-with case of failure. When an axle or shaft fails in service, we examine the fracture, shake our heads, look very wise and say, "Well, this is a beautiful case of re-crystallization." Some of us, however, with very little faith, do not believe there is such a thing as re-crystallization due to vibration. We frankly say it is quite impossible, but again we hide our ignorance by talking about "fatigue." Now we can make a rough classification of ordinary failures of this kind; first, those showing a difference in structure of fracture as shown in Fig. 1, which is a fracture of a $2\frac{3}{4}$ -in. shaft. The inside is very coarsely crystallin; the outside perfectly smooth, and of course we might say that is a clear case where the metal has re-crystallized from the inside, due to vibration; the outside is the fine structure of the material, as it ought to be. Now, as a matter of fact, if we cut a longitudinal section and examine it under the microscope, we find that the structure is uniform and the outside and inside are practically the same. The process of failure is as follows: a slight crack forms on the outside and slowly works its way in; the two faces pound against each other producing a smooth surface and the shaft or axle is reduced to such a cross-section that it is no longer able to stand up, and breaks.

The next slide¹ shows the material under 150 diameters. Mr. Campbell. The ferrite and the pearlite are plainly seen. The structure from the inside and outside is absolutely the same. However, under the microscope we often find that there are spots high in slag, or high in manganese sulfide, as the case may be, and as shown in the next slide. The structure is very fine-grained, characteristic of good material, but shows those longitudinal enclosures, in one case of slag, and in another of manganese sulfide; the slag is usually thinner because it remains plastic longer than the manganese sulfide. We also find certain streakiness



FIG. 1.—Fracture of 2½-in. Shaft.

in the material, as seen in the next slide, which shows the manganese sulfide enclosed in these white streaks of ferrite which have been shown several times to be relatively high in phosphorus. We often find these in failed shafts.

The next class of failures are those which show a rough, irregular fracture throughout. The first sub-division of that is the one illustrated by Mr. Job. The second is the one in which the material went in sound, but subsequently failed. Now these generally show an abnormal microstructure which indicates improper heat treatment.

¹ The illustrations of this and the succeeding slides were not furnished by the author.
—Ed.

Mr. Campbell.

The next three slides exhibit very coarse crystallization, what Mr. Howe calls the prime-austenoid, showing that the material has not been properly worked or was over-heated just before use. The next slide brings that out even stronger, because it shows these large patches of ferrite characteristic of poor material. Here you get the very coarse grain outline and the inclusion of this rectilinear ferrite inside. Such material we know is brittle and will not stand shock.

The material as it ought to be after proper heat treatment is shown in the next slide, representing a very fine-grained mixture of ferrite and pearlite.

On the other hand, very many failures of this kind show correct chemical analysis, as well as excellent microstructure; hence the failure must have been due to improper use. The shaft axle was set wrongly, strained beyond safe limits, and failure occurred in the natural course of events.

Mr. Howe.

MR. HENRY M. HOWE.—I should like to ask Mr. Job if he has conceived of any special stage in the manufacture of that axle in which there is sufficient elongation in the interior of the axle to pass beyond the power of that interior to prolong. It is very evident that the interior of the axle was by some reason or other called upon to elongate to a degree more than it could elongate, and being unable to elongate as much as called upon, it broke, just as a banjo string breaks when you stretch it beyond its power of stretching. It seems to me rather difficult to conceive of any conditions after that material had been forged, at all, when it would not have sufficient power of elongation to undergo the elongation which it was called upon to undergo by anything which happened by any manner of drafts of cold air, or even by being exposed to water on one side or something of that kind. We should recollect that even if we quench that axle in water, we are really throwing the outside in tension, not the inside.

There is a stage, or two stages, in the making of the ingot of the axle when it has practically no ductility. The first is when it is solidifying. Steel of that carbon content would pass for roughly 200° C. through a range of temperature through which it would be mushy, neither solid nor liquid. In that condition it has no ductility, and it is possible that a transverse

crack can arise in that stage. Our experience is that generally Mr. Howe. transverse cracks that arise in that stage, form in the outside not in the interior, for evident reasons. But there is another stage a little later on where internal cracks do arise, which I think is very familiar to those of us who are engaged in the working of steel, and that is when the ingot, while still an ingot, is put while cold into a hot furnace, in which case some care is needed to prevent the forming of such cracks. Isn't that the stage, really, when these internal cracks are most likely to arise? We know, in fact, that if we put a good-size, cold ingot into a very hot furnace we hear sharp clicks, which mean that the ingot has parted on the inside, that we have heated up this outside very rapidly, while the inside has not heated. The outside has expanded because it is heated, and in the effort of the outside to expand, it tears the inside because the outside drags the inside along with it. The inside at that time has perhaps got up to a blue heat, at which it is very brittle, and being unable to follow the expansion thrust upon it by the outside, it cracks. Isn't that really the most probable time when we would look for the formation of these internal transverse fissures, that is, when a cold ingot is put into a hot furnace, if there are people still so base as to do that thing. The inference that I should draw from the existence of such an internal fissure would be that that degree of baseness still existed. The very interesting micrographic features which Mr. Job cites are sufficient to show, I will not say baseness, but a lack of conscientious care, on the part of manufacturers, and that lack of conscientiousness is not incompatible with baseness. If they had sufficient lack of conscience to permit the evident bad practice which occurred, which caused these particular micrographic defects, they may have been sufficiently base to have put cold ingots into hot furnaces, and thereby given conditions which, as we know without the least question, are very likely to cause internal cracks. I should like to ask Mr. Job whether anything that he has thought out would prevent that being a rational explanation.

(Addendum by letter.)—Mr. Kenney has pointed out to me that what is true of an ingot is also true, though less conspicuously so, of a billet or bloom. Putting these when cold into a hot furnace may cause internal fissures in quite the same

Mr. Howe.

way as in an ingot. It is quite true that the degree of elongation which is thus forced on the interior is very much less than the metal would be able to undergo if tested cold in a tension test; but then, as he points out, the power of elongation decreases as the cross-section increases. Beyond this, even a steel which is fairly ductile when tested cold may be rather brittle at a blue heat, and the time when the interior gives way under the elongation thus forced on it may be at this blue heat.

Mr. Gibbs.

MR. A. W. GIBBS.—It occurred to me that the case reported by Mr. Job is one of those fissures of which Mr. Howe has spoken. We had a number of cases in Altoona almost exactly like the one referred to by Mr. Job where a very small hole was seen at the circumference, which was found to lead to a cavity large enough to hold a considerable amount of water. On splitting one of these axles we found not one but several cavities of which one was large enough to come to the circumference. Further investigation showed that this was not so unusual. It was rather disquieting; we found more than we expected and it does not look to me, from those pictures, as if this was at all analogous to a transverse fissure in steel where the surfaces are apparently in contact and rubbing. With us the rough surface formed a lens-shaped cavity in which the fissure was wider at the axis than at the circumference. Our explanation agreed with that offered by Mr. Howe.

Mr. Onderdonk.

MR. J. R. ONDERDONK.—About fifteen years ago, we had an experience similar to that of Mr. Gibbs. We bought a number of new locomotives, in which the driving axles, from the physical tests, showed a very good quality of steel, much better than reported here by Mr. Job. The chemical analysis was perfectly satisfactory, but the axles broke and they showed a cavity such as Mr. Howe has mentioned. The parts that pulled away were separated in some cases as much as $1\frac{1}{2}$ in., and the metal that held the axle together was about $\frac{3}{4}$ in. thick all around the circumference. These axles didn't have holes through them, as Mr. Gibbs has stated, or we might have seen these cavities, but we discovered them after the locomotive had been in service a short time. These particular axles, as I remember it, were rolled, and possibly the effects of the rolling had something to do with producing the fracture. We have not come across such

cases within later years, but it doesn't seem, confirming Mr. **Mr. Onderdonk.** Gibb's opinion, that these fissures are analogous to the transverse fissures that we are getting from time to time in rails.

MR. ROBERT JOB.—Mr. Howe has raised the question **Mr. Job.** as to the exact period in the manufacture of the axle at which the transverse fissure was formed, and he cites the well-known occurrence of ingot-cracks. If, however, the fissure had been formed in the ingot it is certain that the fissure could have been elongated considerably in the subsequent forging of the axle, whereas, as shown in Fig. 1 of the paper, no such condition was found.

The fissure might, however, readily occur provided the axle after being forged were charged cold into a hot annealing furnace, or it could be formed as a result of incorrect treatment after drawing the axles from the annealing furnace.

As was shown in the paper, the steel throughout the axle, and particularly in the central portion, was extremely unsound, and at the center of the axle the ductility was evidently so low that rupture would probably occur after even a moderate stress, which in sound steel would have had little or no injurious effect. The important fact in the case is that the fissure was formed in the mill as a result there of failure to crop to sound metal, and to give correct heat treatment.

It is entirely true, as Messrs. Gibbs and Onderdonk have stated, that an internal transverse fissure of this type is entirely different from those found in steel rails during the past fifteen years, but still to a certain extent there is a clear analogy between the two, since it is common experience that the underlying causes which resulted in the formation of the fissure in the axle also have a decided influence upon formation of fissures or lack of it in rails. To put the matter in another way, if we eliminate from mill practice defective cropping, injurious segregation, strains of manufacture, either mechanical or due to irregular heat treatment, we rarely, if ever, find one of these fissures in a rail.

THE USE OF TURNED SECTIONS IN TENSION TESTS OF REINFORCING BARS.

By E. P. WITHROW AND L. C. NIEDNER.

SUMMARY.

In a qualitative but not quantitative study of the effects of turning specimens of reinforcing bars, it is found that there is an increase in the yield point and ultimate strength which may amount to 10 per cent. This is true of constant-section deformed bars, and even of plain bars.

This is not due to protection from the grips by the large ends of turned specimens for the following reasons: (1) If the specimens are turned to the same diameter for their full length the increase still exists; (2) the yield points of bars embedded in concrete at each end (the stress being applied through the concrete) agrees, approximately at least, with that of the same bar held in wedge grips.

The indications are that the increase is caused by the chemical and structural non-uniformity of the bars.

Since the removal of the exterior steel tends to give a higher reported value for the bars than they would develop in service, it is not good practice to allow the machining of test specimens of reinforcing bars.

THE USE OF TURNED SECTIONS IN TENSION TESTS OF REINFORCING BARS.

BY E. P. WITHROW AND L. C. NIEDNER.

In conducting tests on reinforcing bars at the Municipal Testing Laboratory of St. Louis, our attention has frequently been drawn to the increase in the values of yield point and ultimate strength, caused by the removal of material in machined test specimens. Our experience has shown that in any one type of deformed bar, this increase exists as a variable quantity, the percentage increase often reaching an amount many times greater than the percentage of material contained in the deformations. This phenomenon was found to occur in "constant-section" deformed bars, and even in plain bars.

Since the standard specifications of the Society for Billet-Steel Concrete Reinforcement Bars¹ permit the use of turned test specimens of deformed bars, it is thought desirable to submit certain facts for consideration. It will be noted that the greater portion of the data has been taken from tests on a certain type of "constant-section" bar. The manufacturer's claim for the "constancy of section" of this bar has been practically substantiated by this laboratory, by comparing the elastic moduli of the bar before and after the removal of the deformations. In the computation of these moduli from their respective stress-deformation ratios, the areas of the rough bar was computed from its entire weight per foot, while in the case of the machined bar, its calipered section was used. The tests referred to cover several sizes of both round and square bars, and show an agreement well within 0.5 per cent.

In the study of the phenomenon under consideration several factors were suggested as possibly contributory. The effect of grips, the reinforcing action of large ends, and the effect of non-uniformity of steel due to segregation were among them.

Still another factor, namely, rate of application of load, could not be neglected, because working conditions in most

¹ 1913 Year-Book, p. 80.

laboratories will introduce it secondarily when reduced-area specimens are tested. This factor can cause a variation of indicated yield point and ultimate strength of 1 or 2 per cent. As this amount is of the same magnitude as the percentage of steel in some types of deformations, it should not be neglected.

Many testing machines have only one motor speed with three or four speeds of pulling-heads obtained by gear shifts. One of these is usually the most convenient and is used for this purpose in testing specimens of all sizes. It has been our experience that in the case of turned specimens, if the speed of pulling-head is unchanged, the rate of application of unit stress may be increased to two or three times that found in testing the rough bar.¹

From the data given by Campbell² an increase of 100 per cent in the speed of the pulling-head could cause an increase of 1 or 2 per cent in yield point. In this reference the term "elastic limit" has been used, although the tabulated values are yield points.³ As this gives the speeds of pulling-heads, it is impossible to estimate the corresponding rates of application of load. For reasons previously given, this would be different for each machine and size of bar. In Fig. 1 is shown the effect of varying rates of loading, using a $\frac{3}{4}$ -in. round bar. Each point represents the average of three or four tests. The highest speed shown is below that at which inertia and error of observation could enter to any great extent.

Therefore, the average rate of application of stress has been kept constant up to the yield point in each series of tests shown, with the exception of bar No. 1. The differences are therefore somewhat smaller than those which would be reported for the same material if tested in the usual manner. Owing to the fact that this precaution was not observed in testing for the ultimate strength, the differences in ultimate strength may be too great by a small amount. This is probably not over 0.5 per cent,

¹ There exists, with good lubrication, a slipping of the grips in the pulling-heads as the load increases. This is equal to the product of the wedge ratio and the approach of the jaw surfaces as they take hold of the bar, and is, in general, a function of the total load. If the bar does not slip in the grips, the rate of elongation is therefore the rate of separation of heads minus a function of the total load.

² Campbell, "Structural Steel," p. 253.

³ *Ibid.*, p. 251.

and is due to the change from 5000 to 10,000 lb. per sq. in. per minute, as may be seen from Fig. 1.

In a search for references giving comparisons of tests on machined and rolled sections, some data were found which seemed to neglect the influence of certain variables of such magnitude as would obscure the one sought for. As an example Campbell¹ compares $\frac{3}{4}$ -in. round bars with specimens made by turning $\frac{7}{8}$ -in. bars down to a $\frac{3}{4}$ -in. diameter. Therefore, in the tests here described, such methods were selected as would avoid the ob-

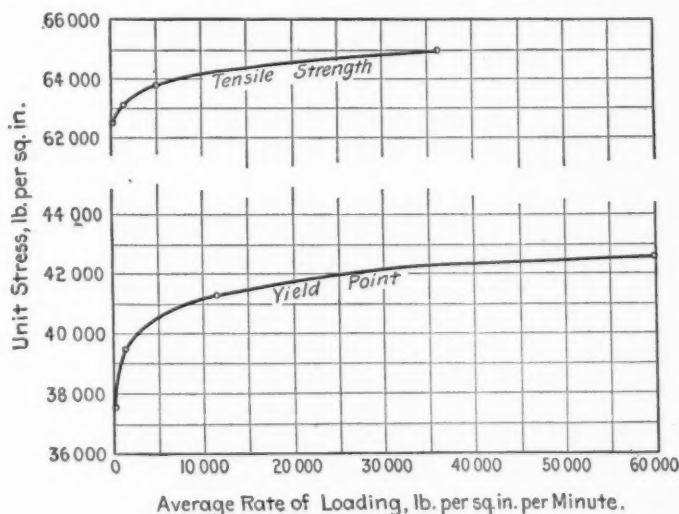


FIG. 1.—Effect of Varying Rates of Loading.

scuring influences which destroy the value of much data of this nature.

The effect of the large ends of machined specimens could be either protection of the specimens, or reinforcing action. That the reinforcing action becomes negligible in its effect on the yield point, when the length exceeds the small diameter, has been shown². As turned specimens used in the present investigation had a length about eighteen times the diameter, the

¹ Campbell, "Structural Steel," Table 84, p. 220.

² Johnson, "Materials of Construction," p. 513.

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TABLE I.—RESULTS OF TESTS.

Bar No.	Type No.	Specimen No.	Yield Point, lb. per sq. in.	Ultimate Strength, lb. per sq. in.	Elongation in 8 in., per cent.
1	1	1	33 170 ^a	55 240	37.8
	1	3	38 410 ^b	55 060	36.8
	4	2	35 250 ^c	59 890	26.3
	4	4	44 450 ^d	59 990	28.0
	1	1	40 320	63 130	30.8
	1	4	39 800	63 420	30.9
	1	8	41 070	63 240	29.3
	1	11	41 090	63 180	29.0
	1	15	41 380	62 990	28.8
	1	18	40 590	63 100	30.6
	Average.....		40 710	63 180	29.9
	4	2	41 620	63 720	26.0
	4	5	44 750	63 720	26.4
	4	9	40 820	63 530	27.0
	4	12	41 140	63 930	26.3
	4	16	39 760	63 670	25.4
	4	19	41 990	63 850	26.5
	Average.....		41 680	63 730	26.3
2	3	3	41 860	63 970	26.3
	3	6	41 610	63 780	27.6
	3	10	41 200	63 560	25.3
	3	13	41 320	63 540	27.6
	3	17	41 410	63 570	26.0
	3	20	41 050	63 710	27.5
	Average.....		41 410	63 690	26.7
	1	2	45 270	62 650	31.3
	1	5	41 910	62 650	31.1
	1	8	44 570	62 630	34.9
	1	11	42 140	62 800	30.3
	1	14	42 870	62 530	30.3
	1	17	45 260	62 630	32.3
	Average.....		43 670	62 650	31.7
	2	3	45 020	62 800	31.3
	2	6	42 610	62 670	28.8
	2	9	44 110	62 700	31.1
	2	12	43 280	62 760	30.5
	2	15	43 960	62 660	30.4
	Average.....		43 800	62 720	30.4
3	4	1	47 440	63 450	27.9
	4	4	44 370	63 370	26.5
	4	7	45 720	63 680	29.8
	4	10	46 430	63 550	28.1
	4	13	45 820	63 730	27.3
	4	16	49 200	63 460	27.8
	Average.....		46 500	63 540	27.9

^a Time of reaching, 25 min. 20 sec.

^b Time of reaching, 1 min. 50 sec.

^c Time of reaching, 14 min. 15 sec.

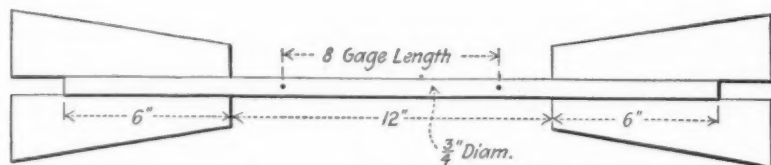
^d Time of reaching, 0 min. 22 sec.

NOTE.—The yield point values were determined by the drop of the beam, verified by strain gage.

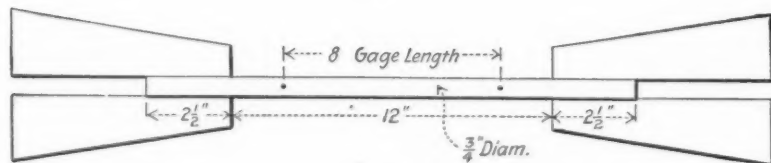
TABLE I (Continued).—RESULTS OF TESTS.

Bar No.	Type No.	Specimen No.	Yield Point, lb. per sq. in.	Ultimate Strength, lb. per sq. in.	Elongation in 8 in., per cent.
4	1	2	44 740	61 170	28.0
	1	5	44 290	61 260	26.9
	1	7	43 680	61 350	29.8
	1	10	43 670	61 390	28.9
	1	12	45 140	61 220	27.8
	1	15	42 640	61 630	29.3
	1	17	43 420	61 980	29.8
	Average.....		43 940	61 428	28.6
	4	1	45 000	66 100	26.5
	4	3	44 980	65 950	25.8
	4	6	47 150	66 560	26.8
	4	9	47 760	66 340	26.1
	4	11	47 650	67 010	25.0
	4	16	46 160	66 730	26.4
	4	18	49 670	66 860	27.8
	Average.....		46 910	66 507	26.3
	5	4	43 410	61 900	29.5
	5	8	43 490	61 980	29.5
	5	13	43 500	61 920	28.5
	Average.....		43 460	61 930	29.0
5	1	1	40 330	55 720	31.8
	1	3	40 200	55 820	31.9
	1	5	39 770	55 660	32.6
	1	7	40 300	56 010	33.5
	1	9	39 770	56 120	33.8
	1	11	38 660	56 160	34.3
	1	13	40 360	55 780	31.3
	Average.....		39 910	55 900	32.7
	3	2	42 010	57 300	29.9
	3	4	41 820	57 910	26.5
	3	6	41 590	57 390	28.5
	3	8	42 700	57 410	30.5
	3	10	42 010	57 760	28.0
	3	12	40 700	57 370	28.1
	3	14	40 340	57 610	30.0
	Average.....		41 600	57 540	28.8
	1	1	36 150	56 900	30.5
	1	2	37 320	56 750	35.0
	1	3	42 100	56 940	31.8
	1	5	39 000	56 850	31.5
	1	7	40 010	56 930	34.3
	1	9	40 610	57 260	31.2
6	Average.....		39 200	56 940	32.4
	5	4	40 390	56 830	32.5
	5	6	37 600	57 800	36.5
	5	8	39 000	57 390	29.5
	Average.....		39 000	57 340	32.8
	1	1	44 200	60 020	31.0
	1	3	46 130	59 900	28.8
	1	5	46 080	60 380	28.1
	1	6	44 610	60 030	28.8
	Average.....		45 250	60 080	29.2
7	5	2	44 280	60 550	29.0
	5	4	47 570	59 230	27.3
	Average.....		45 920	59 890	28.1
	Average.....		45 920	59 890	28.1

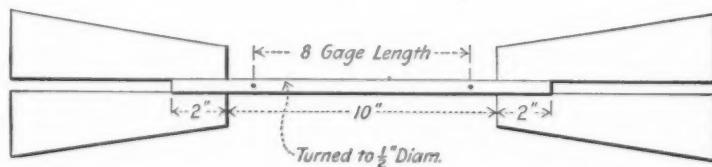
NOTE.—The yield point values were determined by the drop of the beam, verified by strain gage.



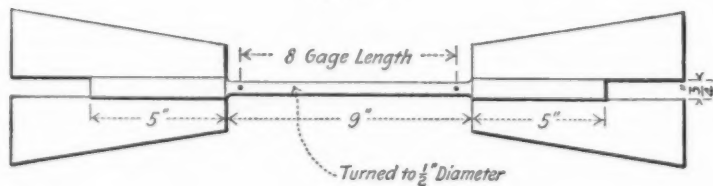
Type 1.



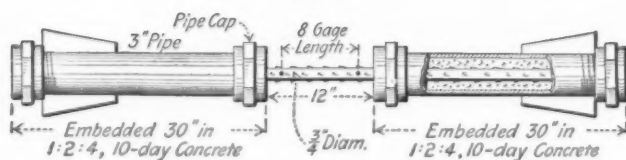
Type 2.



Type 3.



Type 4.



Type 5.

FIG. 2.—Types of Specimens.

only effect of large ends, if it existed, would be that of protection from the grips. The data exhibit no evidence of such protection.

To secure comparable data, several long bars were secured and from each were made various types of specimens. The different types will be designated as Nos. 1, 2, 3, 4 and 5. The dimensions and method of gripping are shown in Fig. 2. The results of the tests are given in Table I.

In the plain bar No. 2, the yield points of types Nos. 3 and 4 agree within about 0.5 per cent. As types Nos. 1 and 3 received an equally severe treatment in the grips, it is evident that the increase of the yield point of type No. 4 over type No. 1 is not due to the influence of large ends. It will be seen also that type No. 3 of bar No. 5, although badly crushed by the grips, shows a yield point 4.2 per cent higher than that of type No. 1. It also shows an increase of 2.9 per cent in the ultimate strength. In bar No. 3 the ultimate strength and yield point of types Nos. 1 and 2 are practically the same, although the specimens of type No. 2 were decidedly flattened by the grips.

On examination of the data from bars Nos. 4, 6 and 7, the behavior of the yield point of specimens embedded in concrete will be found to agree closely with that of the same steel held in wedge grips. Owing to the impossibility of cutting many of these long specimens (type No. 5) from any one bar at our disposal, the following method of comparison is used:

Bar No.	Percentage Increase of Yield Point in Concrete Grips over that in Wedge Grips.	Number of Specimens in Concrete.	Number of Specimens in Wedge Grips.
7	+1.5	3	7
4	-1.1	2	4
6	-0.5	3	6
	Average -0.03		

In the investigation of the influence of grips, this method seems to effect the elimination of practically all other variables. Lack of time prevented a more extended study with type No. 5 of the effect of gripping. The ultimate strengths of specimens of type No. 5 cannot be compared with those of type No. 1,

because the elongation and slip in the concrete grips after the yield point was passed made the rate of application of load uncertain. This was in evidence to such an extent that the intention of keeping it near that of type No. 1 was abandoned, and the bars were broken at a high speed. The comparison of ultimate strengths offers little interest at this point, because the soft grade of steel used showed fracture near the central portion in nearly all cases.

The appearance of a 1-in. plain square bar (bar No. 1), which was being examined microscopically, suggested non-uniformity of metal as one of the causes contributing to the

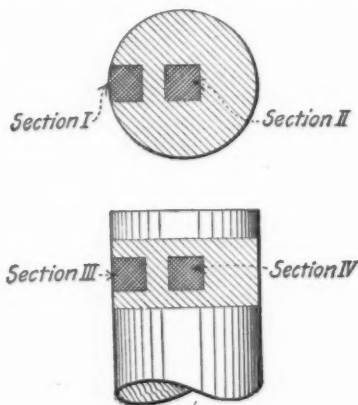


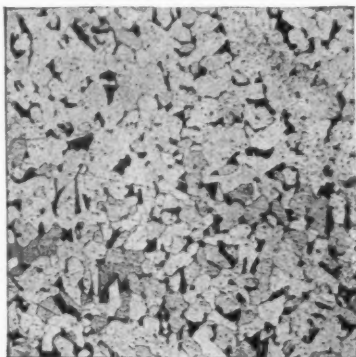
FIG. 3.—Sketch Indicating Location and Notation of Sections shown in Photomicrographs, Figs. 4 to 8, inclusive.

raising of the elastic limit by machining specimens. As will be seen from the photomicrographs¹ (Figs. 4 to 8, inclusive), there is a decided segregation of pearlite in the center as well as a very marked change in the arrangement of grains in lines parallel to the axis of the bar. The macrograph (see Fig. 9) of the entirely etched, longitudinal section, although poorly illuminated at the edges, shows the distribution and the axial arrangement of structure referred to.

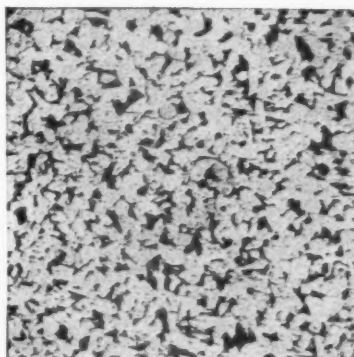
The length of bar No. 1 permitted only four tension tests to be made. Two specimens were tested whole and two were

¹ See Fig. 3 for explanations of photomicrographs.

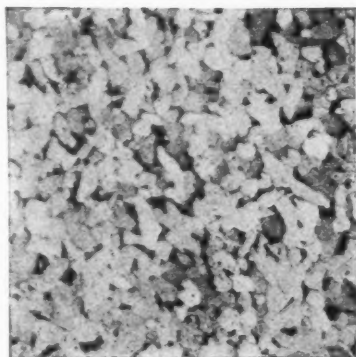
turned for a length of 9 in. to $\frac{1}{2}$ in. in diameter, with full-size ends for the grips. The yield points were intentionally reached at widely different rates of application of load. On account of the small number of specimens, the tests on this bar should only be considered as suggestive.



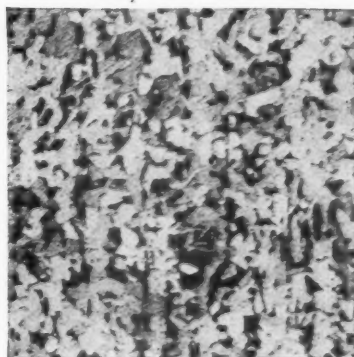
Section I.



Section II.



Section III.

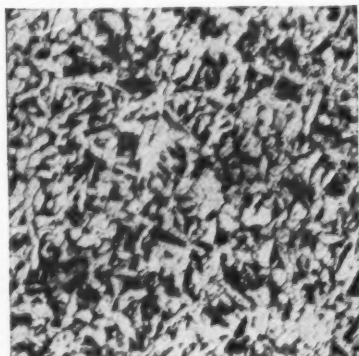


Section IV.

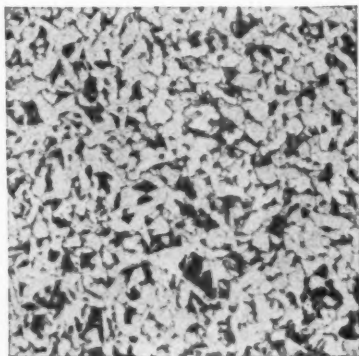
FIG. 4.—Bar No. 1. Magnification, $\times 75$.

The non-uniformity of steel probably offers the principal reason for the fact that most bars, plain and deformed, are known to show some increase when machined to a smaller diameter and that this increase is known to be irregular in bars having the same kind of deformations. An examination of the accompany-

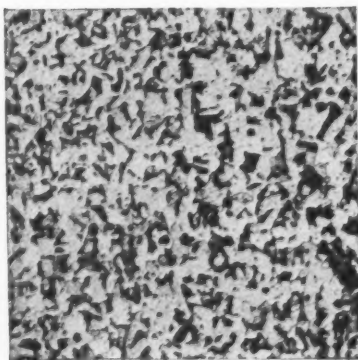
ing photomicrographs shows that in most cases there exists a difference in the structure of the central and marginal portions of the bars. As will be seen from the photomicrographs, the greatest uniformity is evident in bar No. 2 (Fig. 5). The turned sections of this bar show the least increase of yield point and



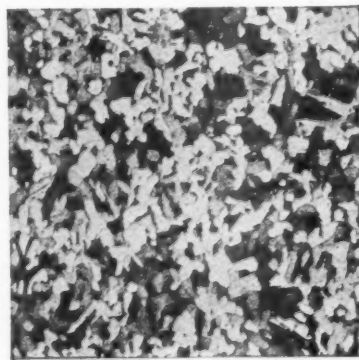
Section I.



Section II.



Section III.



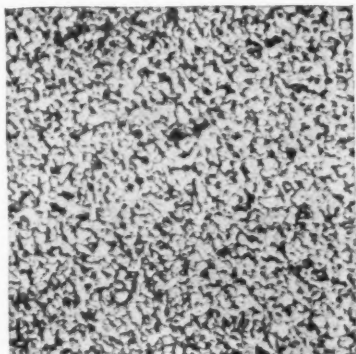
Section IV.

FIG. 5.—Bar No. 2. Magnification, $\times 75$.

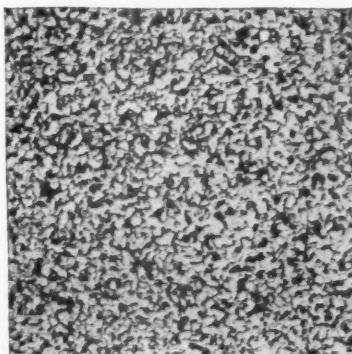
ultimate strength of any bar here reported, about 2 per cent and 1 per cent, respectively.

The effect of non-uniformity of structure is an evidence in the data from bars Nos. 3 and 4 (Figs. 6 and 7). The yield points of the turned section of both of these bars show prac-

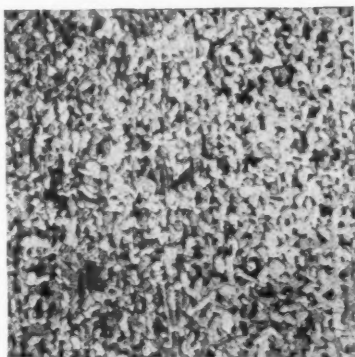
tically the same increase,—6.5 per cent and 6.7 per cent, respectively,—while the ultimate strengths are higher by 1.4 per cent and 8.2 per cent, respectively. The marked segregation of pearlite in the center of bar No. 4 probably explains this difference in ultimate strength.



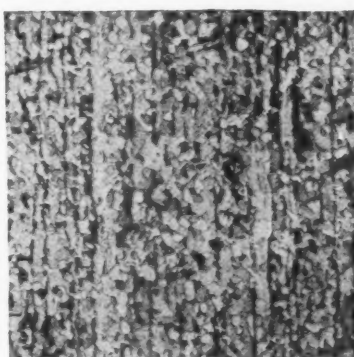
Section I.



Section II.



Section III.

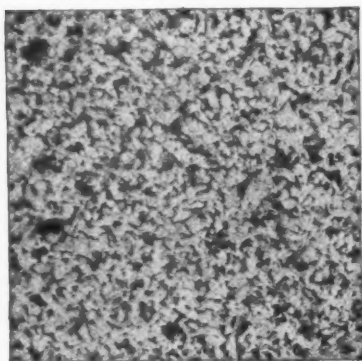


Section IV.

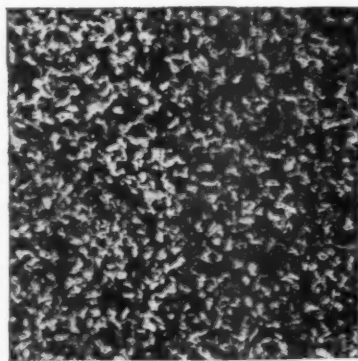
FIG. 6.—Bar No. 3. Magnification, $\times 75$.

That segregation exists over a large central portion of this bar is shown by the macrograph in Fig. 10. This was verified by chemical analysis on material from a $\frac{3}{8}$ -in. hole drilled axially, which analyzed 0.24 per cent of carbon, as against 0.17 per cent of carbon in a sample turned from the margin after the removal

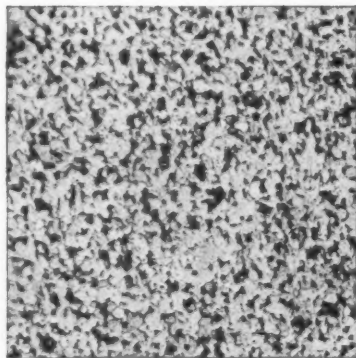
of mill scale and $\frac{1}{32}$ in. of the metal. This observed difference in ultimate strength is not far from that which would be expected to accompany the difference in carbon content.¹ Particular attention was paid to carbon because of the ease with



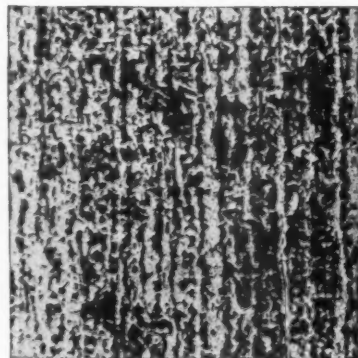
Section I.



Section II.



Section III.



Section IV.

FIG. 7.—Bar No. 4. Magnification, $\times 75$.

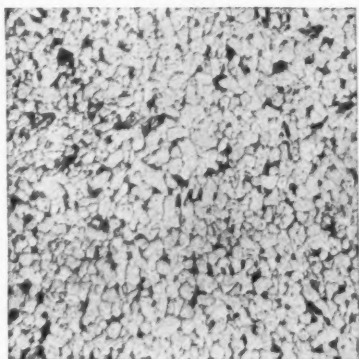
which small variations in its distribution are detected by means of the microscope.

There has been no attempt to determine the magnitude of the effects on the physical properties caused by segregation of various elements. It has been shown, however, that large

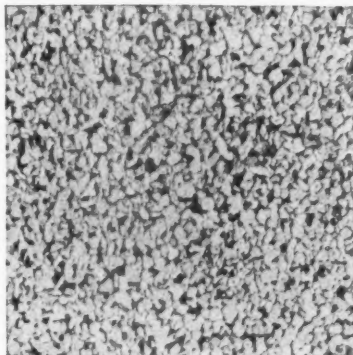
¹ The macrographs (Figs. 9 and 10) show this, besides pearlite segregation.

and variable differences may exist and that these differences probably due to non-uniformity of steel, may, even in a constant-section bar, be greater than the reduction of section due to the removal of inactive deformations.

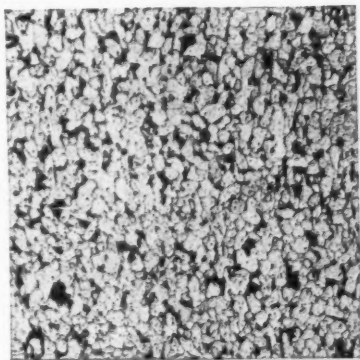
Besides the effects of non-uniformity of chemical composi-



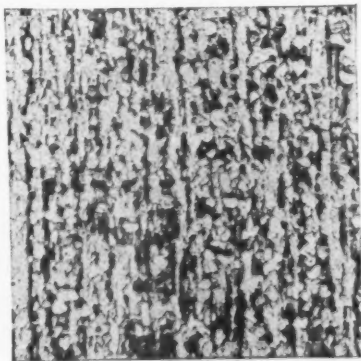
Section I.



Section II.



Section III.



Section IV.

FIG. 8.—Bar No. 5. Magnification, $\times 75$.

tion, non-uniformity of grain size and arrangement could have a marked effect on the physical properties. As the heat and mechanical treatment determine both the grain size and arrangement and also, in a large measure, the yield point, an observed variation in one might be expected to accompany a difference

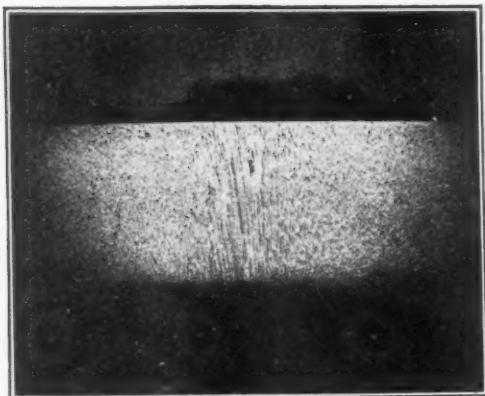


FIG. 9.—Macrograph of Longitudinal Section of Bar No. 1 ($\times 2\frac{1}{2}$ approximately).

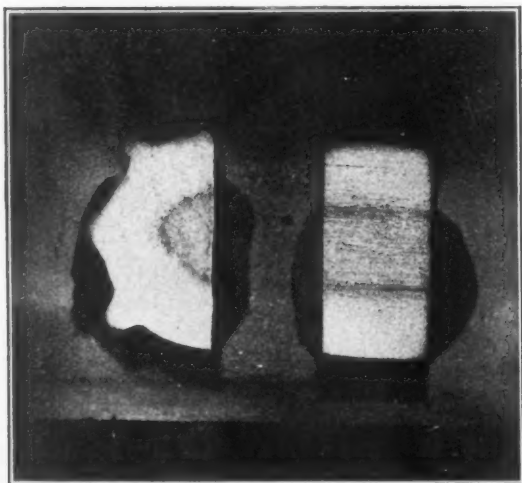


FIG. 10.—Macrograph of Bar No. 4. ($\times 1\frac{1}{2}$ approximately).

in the other.¹ Some of the photomicrographs, especially those of the longitudinal sections, show that the granular arrangement of the marginal and central portions differ greatly. This difference does not require the presence of decided segregation, as will be seen in bar No. 3, where segregation is not evident but where a more decided axial arrangement of grains in the central portion than in its margin is found. The observation of a much greater increase in the yield point than in the ultimate strength of this bar when turned sections are used, seems to point to the reason why this increase in turned sections is rarely the same in both yield point and ultimate strength.

ADDENDUM (*by letter*).

On consideration of the data presented in this paper, it is evident that when the use of the lathe is resorted to in an effort to reduce various deformed shapes to a common basis, homogeneity of steel is assumed without justification.

The desirability of considering the square inch rather than the bar as the unit is also implied. This desirability is by no means obvious. In view of the generally conceded superiority of full-size tests, and the fact that in large structural members the small test unit is used from necessity rather than choice, it may be well to view the reinforcing bar in this light. The bar may be considered as a structural member on which a full-size test to destruction is always available, inexpensive in the amount of material destroyed and, therefore, highly desirable.

We would, therefore, suggest a change in Section 11 (*a*) of the Standard Specifications for Billet-Steel Concrete Reinforcement Bars, page 82, 1913 Year-Book, striking out the provision for machining the bars, and having it read as follows:

"Tension and bend test specimens for plain and deformed bars shall be taken from the finished bars and shall be of the full thickness or diameter of material as rolled."

After Section 8 (*b*), page 81, insert the following:

"The yield point and ultimate strength of plain bars shall be determined by dividing the indicated total load readings

¹ Johnson, "Materials of Construction," pp. 157-158; also Sauveur, "Metallography of Iron and Steel," lesson 5, p. 15.

at these points by the full specified section. The yield point and ultimate strength of deformed bars shall be determined by dividing the indicated total loads at these points by the manufacturer's advertised net section."

The use of the advertised net section in the test computation is justifiable, because the approval of the manufacturer's claim for this section is included in the designer's approval of the shape of the deformed bars. This factor is, therefore, determined before the work is placed in the inspector's hands. That this decision should rightfully come from the engineer rather than the inspector is obvious.

The retest with its bad moral effect is thereby discouraged, as the usual practice at some mills is to test the bars as rolled first, then to make a retest on turned specimens if these fail.

DISCUSSION.

MR. OSCAR U. COOK.—In this paper results are compared Mr. Cook. by using an area obtained from the weight and length of a test bar, which includes excess metal as compared with the net area of the bar. All deformed bars which are rolled have some metal in the deformations which is absolutely of no value to the strength of the bar. Nearly every deformed bar has a net cross-section which can be used in determining the physical properties of the steel. There are some bars which have to be planed or turned to obtain accurate results. The results of the tests given by the authors are of little or no value on account of the areas of original specimens not being true areas. The authors speak of having used a constant-section deformed bar. There is no such thing; the mechanical construction of a deformed bar allows a variation in the cross-section at different points in the bar which may be as much as obtained in the tests cited.

The authors recommend using advertised cross-sections as a basis in commercial testing. There is no more reason for doing this than there would be for basing all tests on advertised areas. Tests are made to show the true values and the physical properties of the material rolled, and one must have correct areas, not false areas, to start with.

MESSRS. E. P. WITHROW and L. C. NIEDNER (*Authors' closure, by letter*).—Mr. Cook has evidently not read this paper Messrs. Withrow and Niedner. with sufficient closeness to obtain a correct understanding of the attitude in which the problem was attacked. He is unwarrantedly harsh in ascribing "little or no value" to the results of the tests. In the summary and conclusion the statement is clearly made that on machining a rod, whether it be a deformed or plain specimen, there is frequently an increase in observed elastic limit and ultimate strength, amounting in some cases to ten per cent above the figure indicated by the unmachined bar.

Since there is a lack of uniformity from surface to center of rod steel it is obvious that a representative sample cannot be obtained by turning off the exterior metal. The simplest princi-

Messrs. Withrow
and Niedner.

ples of sampling must prohibit such procedure, as any one acquainted with this important preliminary feature of testing can certify.

A careful reading of the paper will show that our attention was directed to the subject by the fact that, when the outer steel was removed from deformed bars, the increase in indicated strength was usually much greater than that which could be caused by the mere removal of deformations even if the latter were considered as entirely inactive. Assured by this manifestation that the deformations were by no means the sole cause of the increase, we sought for contributory causes by examining not only plain bars but a certain so-called "constant-section" deformed bar. The latter had deformations of such a nature that its elastic behavior had been found by us to approach very closely to that of plain bars of equal weight per foot.

Several times in our experience with this particular bar our attention has been called to the injustice of the clause in existing specifications which makes the turning of specimens optional with the manufacturer. Although this bar is widely advertised as having a "constant section" for the purpose of design, when it was to be tested it was "deemed necessary by the manufacturer" to resort to turned specimens "to obtain a uniform cross-section" and incidentally to make steel with a 46,000-lb. yield point pass the 50,000-lb. specifications.

Marked evidence of piping and its accompanying segregation has often been found in reinforcing bars examined by us. This may point to the occasional use of "crop ends" by some mills. It is far from our intention here to maintain that piping is injurious to reinforcing bars or that even the general use of "crop ends" in their manufacture would be undesirable from its effect on the bars. The truth we wish to emphasize is that if it should occur its effect on turned test specimens would be to make them not only non-representative but always higher in the observed yield point and ultimate strength.

Mr. Cook should remember that after all it is not the quality of the steel from which the bar is rolled that is important to the safety of the structure, but the strength of the rolled bar considered as a unit. It acts as a unit in the structure, and it should be so tested. If certain types of deformations tend to

develop incipient flaws through sharply re-entrant angles, this will never be known if the material containing these flaws is removed. On the other hand, if a bar having only 95 per cent of the correct net cross-section can carry the intended load with a proper safety factor, the only sources of concern to the engineer should be (1) the slightly reduced superficial area of supplying bond; (2) the lessened cross-section, tending to increase deflection, and (3) the moral question involved through the failure of the mill to supply the contracted weight of steel. Incidentally, the minimum weight per unit length is provided for in the standard specifications of the Society, and furthermore, the use of the advertised net section is now the commonly accepted practice in all parts of the country.

Messrs. Withrow
and Niedner.

In closing, the authors desire to express the hope that the Committee on Steel for Concrete Reinforcing Bars will investigate this subject and, if possible, eliminate from the specifications the objectionable clause which allows, at the option of the manufacturer, recourse to an expedient of such questionable propriety and validity.

NOTES ON CHILLED CAST IRON.

By E. B. TILT.

SUMMARY.

The object is to solve a metallurgical problem on how to make white cast iron stronger and better wearing for car wheels. Illustrations are given of the dangerous defects and of several other common troubles of the cast-iron wheel.

The two main opinions with reference to wheel-iron mixtures are stated, together with representative compositions, as well as a typical wheel analysis of the present-day steel mixture.

Brief consideration is given to the two different materials which form a wheel flange and results of drop tests on gray iron and on white iron of different compositions are indicated. The effect of annealing chilled iron is indicated, as is also the increased strength of white iron when hot. Reference is made to extended data on wheel-foundry practice, as shown by the daily test bars. Results are shown of drop tests on part chilled bars and also of drop tests on the flange of wheels. The possibility of producing a low-expansion iron under brake service is suggested.

A summary is given of the tests of wheel-iron mixtures, which includes different materials. The experiments to date indicate that no combination of pig iron and scrap is an improvement on the standard wheel mixture which contains scrap steel.

NOTES ON CHILLED CAST IRON.

By E. B. TILT.

The consideration of chilled cast iron in the following notes is particularly with reference to its use in car wheels. The problem is to make a sound wheel which will be safe and which will have good wearing qualities. All questions of design, including flange and plate thickness, which also include weight, have



FIG. 1.—A New 675-lb. Master Car Builders' 1909 Design of Wheel

been disregarded, and the question is a metallurgical problem on how to make white iron harder or more resistant to wear, and how to also make the combination of white and gray iron in the flange stronger or tougher.

Fig. 1 shows a new 675-lb. Master Car Builders' 1909 design of wheel, in accordance with the specification with reference to chill. It may be stated that the Master Car Builders' specifications prescribe a method of selecting representative wheels for

test, which are drop-tested for strength and thermal-tested ($1\frac{1}{2}$ -in. ring of fluid iron poured around the tread) to show their capacity to resist the heating action of the brakes. All wheels are inspected for molding defects.

Fig. 2 shows a flange which has been knocked off with a hammer to show the direction of break. This is one of the greatest sources of danger in the cast-iron wheel, though a seam at the throat very often precedes failure.

Fig. 3 shows a typical failure due to overheating by the brakes.



FIG. 2.—Flange Knocked off with a Hammer, showing the Direction of Break.

Figs. 4 and 5 show "brake burns" and "shell outs," respectively. These are minor defects, but to increase the service of the wheels they should be a minimum. It is not known whether metal which shells out or brake-burns easily is dirty or has more impurities than a metal which does not.

There is at present some disagreement among authorities on chilled cast iron as to the best mixtures. Some contend that charcoal iron of chilling quality is necessary (No. 1); others that steel scrap and ferro-manganese (No. 2) serve the same purpose. A representative mixture of each is approximately as follows:

MIXTURE No. 1

Charcoal pig iron.....	70 per cent.
Scrap wheels.....	30 "

MIXTURE No. 2

Coke pig iron.....	45 per cent.
Old wheels.....	45 "
Malleable scrap.....	5 "
Steel.....	5 "
Ferro manganese.....	0.5 " or less.

Wheels made by each mixture meet the specifications, and the advocates of each seem satisfied with the service results ob-

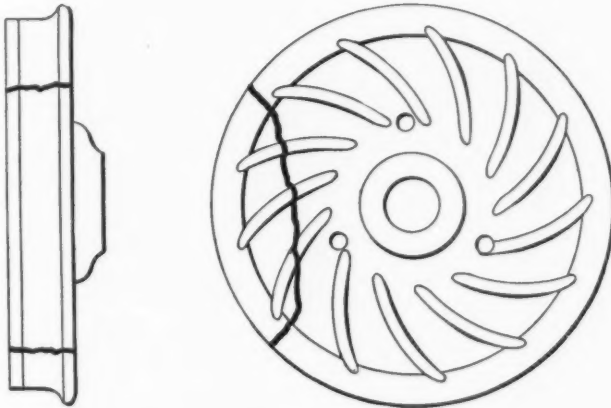


FIG. 3.—Typical Failure due to Overheating by Brakes.

tained. In the author's opinion the latter depends on the final composition, and care of manufacture and inspection. Direct comparisons are difficult to make on account of different methods of keeping records; and comparison of wheels made now of coke iron with wheels made years ago of charcoal iron are difficult on account of changes in the service conditions.

The improvement of the chilled iron lies then in the elimination or variation of certain of the constituents and their compounds, which compose the wheel; or in the addition of other elements or compounds. A representative analysis of the

present-day wheel iron, using coke pig iron and steel, is as follows:

Total carbon.....	3.60 per cent.
Silicon.....	0.60 "
Manganese.....	0.50 "
Phosphorus.....	0.30 "
Sulfur.....	0.12
Iron.....	remainder



FIG. 4.—"Brake Burns."

It is supposed that the metalloids and their compounds, and the compounds of silicon and manganese, compose about 30 per cent of the total volume. Any one of the above can be varied by methods commonly known in foundries, and the best proportions are not definitely settled. Nickel, chromium, vanadium, titanium, and other metals have been used with results not always exactly determined.

It is the writer's opinion, to be confirmed by further experiment, that the amount of total carbon should be low, say about 3.40 to 3.60 per cent, rather than about 4 per cent, as with many white irons. The silicon content will generally be somewhat higher than the manganese, both regulated by the practice of any particular foundry, but in the neighborhood of 0.60 per cent. Good wheels are made with sulfur as high as 0.17 per cent and phosphorus 0.50 per cent.

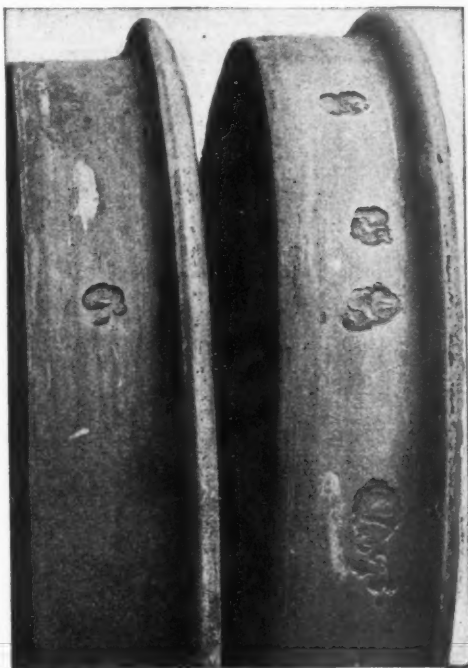


FIG. 5.—"Shell Outs."

An examination of Fig. 1 shows the flange to be white iron in part, and Fig. 2 shows the broken flange continued down through the gray iron, which is the usual direction of breaking. Fig. 6 shows stress-deformation curves for gray irons from the sources noted, and for comparison the curve for the common steels is included. A curve for white iron is shown where it is thought it might be found, and from a consideration of this it

is evident that the flange strength is dependent upon two substances. The first is the white iron which carries almost all the load until rupture takes place in it, when the gray-iron back takes the load. We have thus successive failure of the white and the gray iron, and it is evident that if both could be of the same elastic strength an improvement would be made. A consideration of this suggests no possibility of attaining such a desideratum, so that a possible solution is to have as much white iron (a portion being necessary for wear) as can possibly be

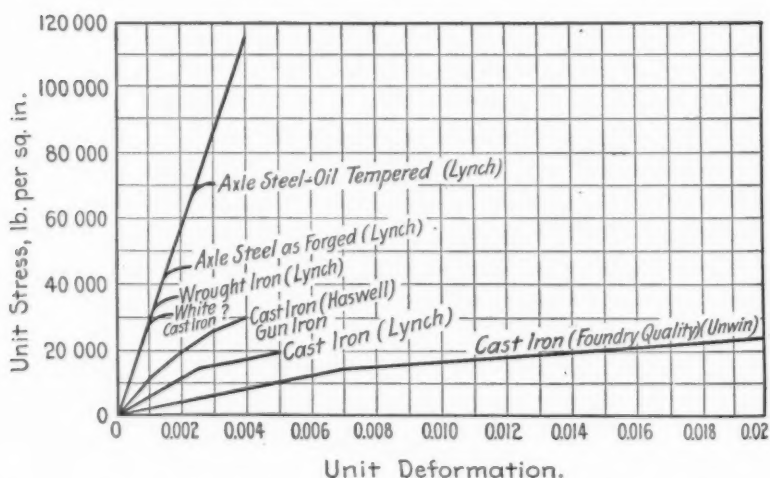


FIG. 6.—Stress-Deformation Curves for the Common Steels and Cast Iron.

allowed, and thus also to have the gray iron as hard as possible without its being too brittle in the plates.

In order to see whether the strength of white iron varied appreciably in our regular practice, it was decided to test chilled circular rolls, 2 in. in diameter (barber rolls), and partly chilled rectangular bars $1\frac{1}{2}$ by $2\frac{1}{2}$ in. The combination chilled and gray-iron bars are similar in structure to the flange of a wheel. These were drop-tested on 10-in. supports with a tup of 42.3 lb., caught on the rebound. Fig. 7 shows the ends of two rolls, also a regular wheel foundry chill block taken to show the amount of chill at each tap, and of one of the partially chilled test bars. Note the difference in the coarseness of the chill on the roll on the

left, which was weak. On account of the elastic limit and tensile strength of chilled iron being practically the same, the drop is started at 8 in. and increased 1 in. at a time until the bar fails. The difference in height of the drop to break bars of widely different mixtures, in chills and in green sand, is given in Table I.

The wheel iron is the strongest, whether cast in chiller or in green sand. An interesting point is that when the wheel-iron bars are annealed in the pits with the wheels (a period of 4 days), the average height of drop decreases from 15 to 12 in. This would suggest that unannealed flanges are the strongest. When the unannealed bars are heated to 500° F. and tested at that

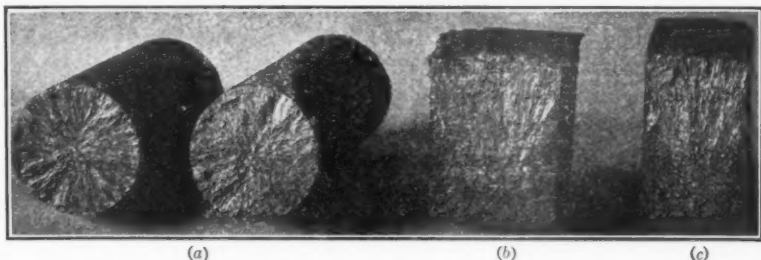


FIG. 7.—(a) Ends of Two Chilled Rolls.
(b) End of Regular Wheel Foundry Chill Block.
(c) End of Partly Chilled Rectangular Bar.

temperature, the average height of drop is increased from 15 to 20 in. This suggests less liability to strip flanges by blows under heavy brake service, when the wheels would be hot.

Records were kept for a number of months operation of the wheel foundry, by plotting the results of drop tests of chilled rolls, of 1½-in. square transverse test bars, of the chill on chill blocks, and of the hardness of the chilled rolls. A consideration of these results seems to show that the greatest strength of the white iron is accompanied by the greatest hardness, though the variation due to composition, or any other cause than flaws in the test specimens, is small.

Representative drop tests on the half-chilled 1½ by 2½-in. bars with the white iron down or in tension, are shown in Table II.

The difference in the amount of chilled iron has very small effect on the strength. This is in agreement with the foregoing, as the difference in amount is at or near the neutral axis and the strength of the metal at the surface varies very little.

In order to get a direct measure of the strength of the flange we have been drop-testing the flange of the wheels on a small drop-testing machine, with a 25-lb. tup falling on a striking block 2 in. wide with a face having the contour of the throat of the flange. The wheel to be flange-tested is bored to a tight fit on a mandrel which forms a part of the foundation of the testing machine. After some experimenting 6 ft. was taken as the height from which to drop the weight, and by taking one given weight and

TABLE I.—HEIGHT OF DROP TO BREAK BARS OF WIDELY DIFFERENT MIXTURES.

Quality of Iron.	Silicon, per cent.	Combined Carbon, per cent (estimated).	How Cast.	Chill.	Height of Drop to Break, in.
Wheel iron.....	0.68	3.50	Chiller	Completely	15
Wheel iron.....	0.68	1.00	Green sand	None	27
Cylinder.....	1.27	Chiller	Around edge	14
Cylinder.....	1.27	0.90	Green sand	None	21
Machinery.....	1.60	Chiller	Mottled throughout	10
Machinery.....	1.60	0.60	Green sand	None	18

design of wheel, we have found that for the same mixture the number of blows to break off the flange is inversely as the chill. This means that a soft wheel has the strongest flange. In service we have more stripped flanges in soft wheels than in hard, but this is due to the development of cracks in the throat of the flange. This suggests that the design of the wheel and care in securing enough chill are more important than differences in quality or source of the iron.

A possible improvement in wheel iron, to avoid trouble with broken wheels, is the making of an iron with a low coefficient of expansion under brake action. Heavy braking results in heating of the tread, with consequent expansion and pulling apart of the plates; it is the ability to withstand this service that is

shown by the thermal test. Thermal tests on a large number of wheels show an increase in diameter of about $\frac{1}{16}$ in. after the iron ring has been on two minutes, failure taking place after an increase of anything from $\frac{3}{64}$ in. up, depending upon how much white iron or combined carbon is in the plates of the wheel. The harder the wheel the more readily it is broken in the thermal test. Part cooling curves for wheel iron are shown in Fig. 8, and it would seem that shrinkage is fairly uniform for most irons of similar composition after solidification has taken place. Note the expansion in cooling of one wheel mixture which was soft. A wheel poured from this would have the flange and tread chilled and set at once and the expansion due to the gray iron behind might cause some checking of the chilled iron, which

TABLE II.—DROP TESTS ON HALF-CHILLED BARS.

		Depth of Chill, in.	Height of Drop to Break, in.
Maximum	High-Chill Bar...	1.30	18
	Low-Chill Bar...	0.95	18
Minimum	High-Chill Bar...	1.05	12
	Low-Chill Bar...	0.60	12
Average	High-Chill Bar...	1.20	13.5
	Low-Chill Bar...	0.75	14

would show as a defect developed later in service. So too it has been thought by some that brake heating produces circumferential throat seams, due to expansion of the tread which is resisted by the flange. If low-expansion iron is a possibility this tendency would be lessened.

Whether unclean metal or metal high in sulfur or manganese is the most likely to shell out the writer cannot say, and it is hoped to get some connection between this trouble and the different coefficients of expansion of the compounds forming the metal.

Experiments have been in progress for some time and many test heats have been taken off. The fuels used have been high and low-sulfur cokes and anthracite coal, and the mixtures have varied from all-scrap wheels to mixtures with charcoal iron and malleable and steel scrap. Six test wheels have been cast from

each test heat, of which two each have been thermal, drop and flange tested. Up to the present there has been no improvement on our standard mixture containing scrap steel. There have been only a few analyses and the tests, to show improve-

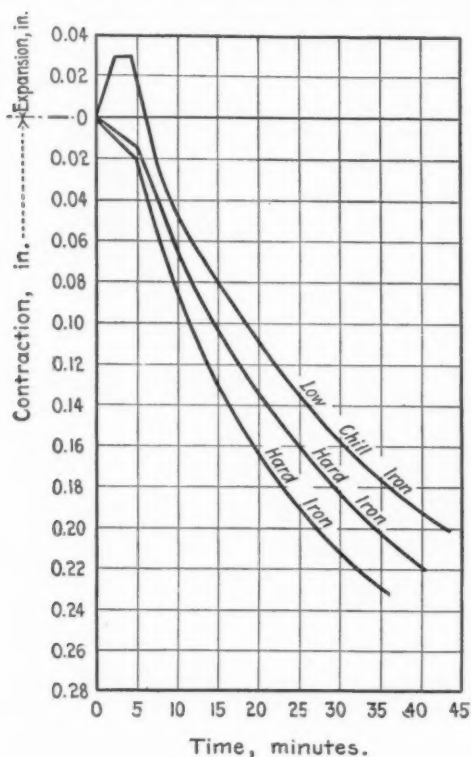


FIG. 8.—Shrinkage Curves for 1-in. Square Bar, 12 in. long, Wheel Iron. Cast in Sand.

ment, have been confined to those ordinarily made on wheels and bars, as given above. Our test work is to continue and it is our expectation that we will find mixtures and perhaps methods to improve the cast-iron wheel.

DISCUSSION.

MR. T. D. WEST.—Those experienced in making chilled Mr. West.
iron castings know that this product requires delicate and difficult treatment. Any system of tests for chillable irons ought to take note of the most minute changes in the metal. Mr. Tilt states in his interesting paper that his object is to solve the metallurgical problem of making white cast iron stronger and more durable as to wearing qualities for car wheels. To determine whether the strength of this white iron varied appreciably in his regular mixtures he used all-chilled, 2-in. diameter bars in connection with partly chilled, $1\frac{1}{2}$ by $2\frac{1}{2}$ -in. rectangular bars. If a student of this subject will refer to my paper, No. 1350, presented at the Spring Meeting of the American Society of Mechanical Engineers in 1912, he may be surprised to find that my tests showed that as a rule the results from partly chilled bodies, such as Mr. Tilt's rectangular bars, are very erratic, since they are affected by the slightest variations in the temper of the sand, the hardness of ramming, the temperature in pouring, etc. Of course Mr. Tilt's rectangular bars were utilized chiefly for regulating his mixtures; nevertheless, I contend that in thus utilizing these bars, too great care cannot be exercised in striving to have all conditions as nearly constant as possible.

The results of many tests showing the erratic quality of partly chilled rectangular bars, lead me to the conclusion that a more reliable method than the use of a combination of round and rectangular bars should be used in studying the characteristics of chillable irons with a view of making white cast iron stronger, and obtaining wheels of better wearing qualities. In experimenting towards these ends, I was forcibly brought to the conclusion that in order to determine the true difference between the physical qualities of white and gray cast iron of the same metal, I would have to use the same form of test bar and have all conditions alike for both bars, except the one which would give an all-gray and an all-chilled bar from the same metal, poured at the

Mr. West. same time. The method I devised is shown in my paper on "A Suggested System of Test Bars for Chillable Iron," presented at the Sixth Congress of the International Association for Testing Materials in 1912. I am convinced that this method of testing chillable irons to assist in obtaining better wheels, etc., would be a most excellent one for the American Society for Testing Materials to adopt as one of its standard tests for determining the physical properties of cast iron. In that connection it may be said further that when a bar is of such a size that the metal is liable to assume a mottled form, it is very likely to go further and become almost white, and that with the same iron, temperature of metal and character of mold, the ultimate tensile strength is just as likely to be 7000 as 5000 lb. per sq. in. This sensitive condition should be avoided in making comparisons between all-chilled bars and all-sand-cast bars of the same size, form and metal. To do this it is necessary that the mold should be sufficiently large to prevent the metal in the sand-cast bar from assuming the mottled form, and at the same time sufficiently small to insure an absolute chilling to the center in the case of the all-chilled bar. Much experimenting may be necessary to determine the best diameter of bar to insure these results. My own experience in that connection is described in the paper previously referred to.

Mr. Lesh. **MR. I. B. LESH.**—I think Mr. Tilt has some definite data, covering probably not the experiments or work being done at present, but work done some years ago. I believe it would be of interest to the Society to show what has already been accomplished in the line of his present work.

Mr. Tilt. **MR. E. B. TILT.**—The information referred to is contained in a paper by Mr. H. H. Vaughan, Assistant to the Vice-President of the Canadian Pacific Railway Co., read last January before the Canadian Railway Club, at Montreal. I would refer you to that for full data as to the increase in average life of our wheels and the elimination of failures of various kinds. Our company has been keeping very careful records of the performance of our wheels. I can say that we have no fear of the cast-iron wheel, and that we have come to regard it as quite as good a wheel as we have. It is true we are not using it under our passenger cars or under our passenger engines, but we have no misgivings about

the reliability of the wheel. Flange failures have been so nearly eliminated, that broken and chipped flanges together run from 0.01 to 0.02 per cent the first year of service and increase to only 0.2 per cent for the fourth year; in fact they have been more than cut in half. Broken wheels are very few in number, rarely amounting now to 0.003 per cent. So, too, the average life of the wheel has increased from less than five years to nearly six years. This has been accomplished not only through the improvement of the iron itself, but also through better inspection by elimination, both at the foundry and on the road. Mr. Tilt.

Replying in part to Mr. West with reference to the differences we might expect in our chilled bars, we have assumed that all white iron is the same. Though we recognize that there might be considerable differences in white irons from different foundries and sources, there is probably very little difference in our wheel foundry operations from day to day, and therefore small difference in the white iron. If that assumption is reasonably true then comparisons of all-chilled bars, such as the barber rolls, may be made. So, too, in the partly chilled bar, any bar with a given chill is compared with any other bar of the same chill, even though cast on different days. All possible safeguards to produce bars under uniform conditions were taken.

MR. J. E. JOHNSON, JR.—With reference to the saving of replacement, I was given some data from the wheel foundry of the Chicago, Milwaukee and St. Paul Railway Co. They made the claim that they get about 85,000 to 95,000 miles out of a wheel, as compared with 35,000 to 45,000 miles for wheels bought to ordinary specifications. It seems they buy charcoal iron at any price necessary to get the quality they want. They do not consider taking the wheels of foreign cars, melting them and putting them on their own cars; they simply throw them all out; they will not let a piece of foreign wheel go back, but they buy the best of charcoal iron. I have seen charcoal iron in their yard that cost them \$40 a ton. They also do what Mr. Tilt said: they have a great box of test-bar samples, over 50 representing a day's run. After looking at them and then having them transposed by someone else you can not tell, on looking back, which ones had been touched. I had never before seen such excellent foundry work, and they were also using the utmost Mr. Johnson.

Mr. Johnson. care in inspection. There is no other answer to the chilled iron wheel problem but the use of first-class material and careful inspection.

Mr. MacPherran. MR. R. S. MACPHERRAN.—What is the relation of hardness to wearing properties? Is there any way of determining absolute hardness?

Mr. Tilt. MR. TILT.—I do not think I can answer that. We came to one general conclusion, that the strongest iron we got was the hardest. The hardness tests were made with the scleroscope, but they have not been as uniformly high as we had reason to anticipate. As to ultimate wear and hardness, I am afraid I cannot give anything on that. It is very difficult to follow the wear. It is evident that some laboratory way of testing the wear of white iron will have to be developed.

I should like to take issue with Mr. Johnson on the use of charcoal iron. It may be true that the Chicago, Milwaukee and St. Paul Railway Co. is getting exceptional wear out of their wheels, but they will have to show more positively that it is on account of the use of charcoal iron. I believe in the selection of a good type of truck and good upkeep, inspection on the road and other inspection tending toward the elimination of bad wheels, careful brake service and all that tends towards good operation, as being more important than a particular brand of iron. The Chicago, Milwaukee and St. Paul Railway Co. is an exceptional railroad inasmuch as they keep the majority of their cars on their own road, but we want something more positive that charcoal iron is essential to producing good car wheels than mere belief that it is essential. I believe that coke iron will produce as good car wheels as charcoal iron, and that this will be shown by any test, either in the laboratory or in service. We are getting as good or better results to-day with coke iron and steel scrap car wheels as were ever gotten with the use of charcoal iron on our road.

Mr. Lesh. MR. LESH.—I wish very briefly to support what Mr. Tilt has said. In a rather wide foundry experience, I have been unable to find that any two or more cupola mixtures, if of the same chemical composition properly melted and properly poured, will produce castings of different physical qualities, regardless of whether these mixtures are composed of charcoal pig iron,

coke pig iron or scrap, or whether they cost \$40 or \$14 per ton. **Mr. Lesh.**

In fact the most successful example I know of in the improvement of cast-iron products was secured, not by the use of charcoal pig iron, but by discontinuing its use and substituting high-grade coke pig iron strictly on the basis of its chemical analysis. This happened to be in chilled car wheel mixtures, and together with improved foundry practice and inspection, resulted in a reduction of 65 per cent in replacements of failed wheels. This would not prove that coke pig iron is in itself better than charcoal pig iron, but together with many other similar cases it indicates that the relative value of either is determined by their chemical analysis, and not by the name or cost per ton.

Chilled car wheels admit of most careful supervision in manufacture and most exacting tests at inspection. If this supervision and these tests are consistently followed, it is very difficult to get inferior wheels into service. If the mixtures are carefully calculated to proper chemical analysis before being charged into the cupola, if the cupola and foundry practice are right and the tests made scientifically and without prejudice, there is in my judgment a much better chance of getting good wheels than if dependence is placed on fancy brands of pig iron at fancy prices.

MR. G. L. FOWLER.—I should like to corroborate **Mr. Lesh's** statement in regard to the use of charcoal iron. A short time ago I made a tour of several of the largest foundries in the country, and I failed to find any of them that were using charcoal iron to any great extent. The whole process of car wheel manufacture has been changed in the last 25 years, from selecting the pigs by fracture, looking at them with one eye and saying "That's good and will carry about so much scrap," to piling the pig up in the yard on a chemical analysis so that an office boy can go out and make the mixture and put it into the cupola and get as good results as the foundryman. The thing is to melt your iron properly, pour it at the proper temperature and do the proper annealing; yet I know of one foundry that is getting first-class results and doing it by the old process of throwing the pig iron off the top of the car and seeing it break and piling it up by the fracture and making car wheels by fracture, but that is the only company I know of which does that kind of work. The

Mr. Fowler. rest are working entirely on chemical analysis and are getting excellent results.

In regard to the cost of the cast-iron wheel for repairs, I know of one large railroad for which it costs the contractors who supply their wheels 4 cents a wheel on the average for replacement. That is about the average cost to the manufacturer.

Mr. Johnson. **MR. JOHNSON.**—In regard to some of the statements that have been made here, I have nothing to offer in rebuttal except the fact that I have taken irons of identical analysis, as nearly as they could be picked out of an assortment of about 20,000 tons in the yard, and that I have put a weak and a strong one in two separate crucibles, melted them down at the same time, poured them in the same molds, with the result that the iron that went in strong with high chilling power, came out the same, and the iron that went in weak came out weak in every case. There is no doubt in my mind, in spite of what has been said, that there is an enormous difference in the strength of iron and in its chilling power which does not depend on its ordinary analysis, and I can make a demonstration of that fact in almost any way, where the materials are available.

As to charcoal iron and its use or non-use, there has been made in the last 15 or 20 years an enormous amount of charcoal iron that was not as good as coke iron, and was very irregular. It was made to burn up the charcoal, which was a by-product. This objectionable condition has done a lot to hurt charcoal iron in this country. Some of these irons are extremely weak, have no chilling power and are not fit to put into a car wheel where strength or chilling power is required; but given a good charcoal iron you can make castings that are a great deal stronger and tougher than can be made from the best coke iron produced. If you want chilling power you can get a great deal deeper chill out of a good charcoal iron than you can out of the best coke iron with the same silicon. That is a matter capable of absolute demonstration.

The Chairman. **THE CHAIRMAN (MR. RICHARD MOLDENKE).**—It will be necessary to close the discussion at this point. Mr. Johnson is undoubtedly right in what he has presented, but Mr. Tilt is also correct in claiming that good coke irons, with steel additions, will give results equal to the best charcoal irons.

CONSIDERATIONS AFFECTING SPECIFICATIONS FOR WROUGHT NON-FERROUS MATERIALS.

BY W. REUBEN WEBSTER.

SUMMARY.

It is the purpose of this paper to discuss the main factors which require consideration in connection with the writing of specifications for wrought non-ferrous materials.

A very wide range of properties can be imparted to these materials by variation in their constituents, by different amounts of cold working, and by different degrees of annealing. By these means, a very wide range of requirements can be successfully met. In this connection, it is pointed out that the methods of defining suitability for specific usage are largely empirical.

The methods employed in producing the copper-zinc alloys in sheets and strips, rods, wire, and seamless tubes are briefly described and the effects of the various operations upon the properties of the finished material are discussed. The effect of varying proportions of copper and zinc, of varying amounts of cold rolling, and of varying degrees of annealing temperature, are shown by means of curves. The effect of certain impurities on physical properties is also considered.

Attention is called to the large number of variables involved and to the difficulty experienced in controlling them within close limits, more particularly because of the fact that the unit heat is comparatively small.

Finally, the necessity for accurate knowledge is pointed out as to the relation between physical and chemical qualities and the ability to best meet the requirements of specific use on the one hand, and on the other hand of the limits within which it is possible to control the variations of these qualities.

CONSIDERATIONS AFFECTING SPECIFICATIONS FOR WROUGHT NON-FERROUS MATERIALS.

BY W. REUBEN WEBSTER.

INTRODUCTORY.

It is the purpose of this paper to enumerate and discuss some of the factors which affect the construction of specifications for wrought non-ferrous materials. While the use of these products in construction is large and important, the variety of uses is so extensive that with some exceptions not many data are available to the engineer desirous of defining his necessities.

Attention will be particularly confined to the copper-zinc alloys as representing the most comprehensive group of materials that are fabricated by wrought processes into sheets and strips, rods and bars, brazed and seamless tubes, and wire.

PROPERTIES OF COPPER-ZINC ALLOYS.

Copper and zinc alloy to form malleable and ductile materials in all proportions from about 55 per cent up to pure copper, and over this range the products exhibit a normal variation of physical properties which is very great. Moreover, the properties normal to any particular proportion of the two constituents can be greatly modified by the processes of cold working and heat treatment. Other modifications are produced by the presence, either accidental or intentional, of other elements, more particularly lead, iron and tin. The effects of these vary in turn with the percentage of copper present and may be harmful or beneficial according to the purpose for which the material is required. Lead causes it to work freely under a tool but decreases ductility, the latter effect in turn becoming more pronounced with decreasing percentages of zinc. Tin, on the other hand, decreases ductility greatly in the presence of considerable percentages of zinc. It is this very wide range of properties which can be imparted to the copper alloys in the wrought form, together

with their non-corrodibility, which gives them the great value which they possess. At the same time, formulas have not been deduced which will accurately correlate components and properties to uses, although experience has determined with more or less correctness those things which should be avoided when any known purpose is to be met. Fig. 1 shows the variation in physical properties with the proportion of copper and zinc when cold-rolled and annealed.

It is general practice to produce the alloys by melting the

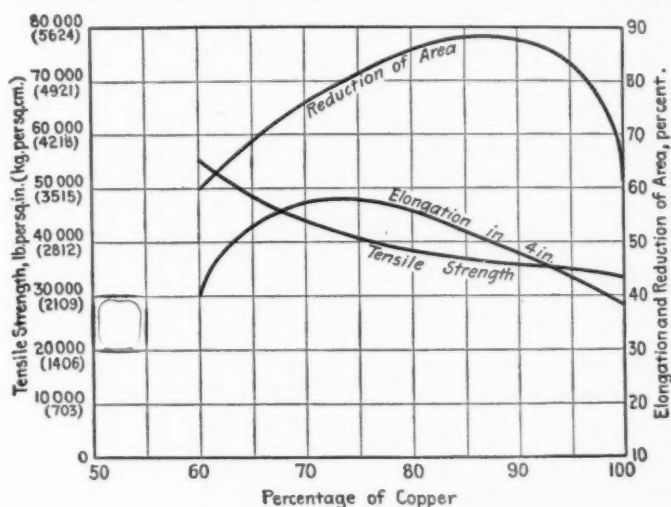


FIG. 1.—Curves showing Variation in Physical Properties of Cold-Rolled and Annealed Copper-Zinc Alloys, due to Varying Percentages of Copper and Zinc.

constituents in crucibles containing about 200 lb., so that this quantity represents the unit heat. To determine all the important constituents of the product of each crucible charge is manifestly commercially impracticable, so the range of tolerable variations as determined by individual analysis must of necessity be comparatively large.

VARIATIONS IN COMPOSITION.

Departures from a desired composition as influenced by variation in the copper are not difficult to control, because ingot

copper is produced in large furnace charges, permitting of accurate sampling and determination. Such, however, is not the case with zinc, each plate of which (about 40 lb.) not only shows an individual lead content of its own, but on account of the fact that lead and zinc do not alloy, will show widely different proportions in various parts of the same plate. Moreover, zinc is highly volatile, so a certain proportion of it is lost during the alloying process. This amount is difficult to control within close limits. It is governed by the proportion of zinc in the charge, the temperature of the crucible contents at the time the zinc is introduced, and the time elapsing between the introduction of the latter and the pouring of the crucible contents. It is evident also that some variation is due to the limit of error in weighing the charge, and that the large number of individual weighings necessary in producing any considerable quantity of brass affords an opportunity for occasional error. More or less variation also occurs in different parts of the same bar. As a result of all these opportunities for variation, the departure which any individual bar or ingot may show from the desired composition is considerable, its limit being the sum of all the variations due to each of the causes enumerated.

In addition the errors of chemical analysis need consideration. The Bureau of Standards recently prepared an accurate sample of sheet brass and made a careful determination of its constituents. Portions of this sample were sent to eleven different chemists accustomed to this class of work for check analyses. The extreme variations reported to the Bureau showed for copper 0.15 per cent of the total copper as determined by the Bureau, for zinc 0.89 per cent, for lead 5.2 per cent, for iron 13.8 per cent and for tin 10.2 per cent. It should be borne in mind that these determinations were made under conditions conducive to much more than commercial accuracy.

Three qualities of copper are ordinarily employed, namely, lake, electrolytic, and arsenical. The differences in the resulting product due to the employment of any one of these three grades is not definitely established. In the early days of the industry, electrolytic copper was liable to contamination, but such a condition no longer exists. Several grades of spelter are in general use, the difference being due primarily to the lead content.

It is customary to make up the mixtures with certain proportions of scrap. The term scrap as here employed is not at all synonymous with junk. Good practice tolerates scrap only with a known history and of known composition. Properly safeguarded in this respect, the use of scrap is advantageous. It is much more difficult to produce a thoroughly homogeneous mixture when the entire charge is composed of unalloyed copper and spelter.

INFLUENCE OF THE CASTING PROCESS.

The casting process, besides being the cause of variations in the composition of the finished product, affords other opportunities for variation in the quality. When properly performed, the bar, billet or ingot should be free from blowholes, gas cavities or dirt in the interior, and from cold-shot or other surface imperfections on the exterior. Improper or unskilled casting profoundly affects the strength and ductility of the ingot.

The surface of molten brass becomes instantly covered with a thick film of oxide on exposure to the air, and this oxide must be prevented from becoming incorporated in the solidified metal. Two instances will illustrate the importance of proper casting. Ingot copper of the finest character, as received from the refining furnace, is neither very ductile nor malleable when cold, but when properly melted and cast from a crucible, it becomes highly so. A properly cast billet should show no pipe at the upper end, and when improperly cast a considerable pipe may occur. Fig. 2 shows a section of the upper part of a billet properly cast, and Fig. 3 of one improperly cast. The cut across the top of the billet in Fig. 2 shows the portion ordinarily discarded.

The crucible charge is cast into several different forms according to the use for which it is intended. If for sheet brass or strip, it is cast into bars usually about 1 in. thick and of a width varying from 4 to 14 in., according to the width of the sheet required. The length is such that the standard crucible charge will make two bars. Wider sheets are produced by rolling the bars crosswise in the rolls until the required width is reached, or by cutting a partially rolled bar into lengths of approximately the required width and cross rolling to the finished thickness, according to the practice generally followed in rolling sheet steel.

Mixtures which admit of hot rolling are cast usually of greater cross-section, and of varying weight according to requirements. The extrusion process employs a billet 6 in. or more in diameter, weighing 150 lb. or more, according to the power of the machine employed.

OPERATIONS EMPLOYED IN MAKING SHEET AND STRIP.

Considering first the bar intended for sheet or strip, the various operations affecting the finished product will be discussed.



FIG. 2.—Section of a Properly Cast Brass Billet.

After being cast, the bars have their gates or shrinkheads cut off. At the same time, the cut and exterior surfaces are carefully scrutinized for evidences of improper casting, and should any be found, the unsatisfactory bars are cut up to be recast. From the shears, the bars are then placed in an annealing furnace, heated to redness, withdrawn from the furnace and allowed to cool. They are then given several passes through the breaking-down rolls, being reduced 40 to 50 per cent in thickness. After breaking down, they are again annealed and overhauled.

This last operation consists in scraping or cutting the surface of the bar with a reciprocating or rotary tool for the purpose of removing surface defects. This process affords opportunity for an efficient inspection, any defects in the casting being readily observable.

Rolling and annealing operations now alternate until the material is reduced to the desired thickness. The former opera-

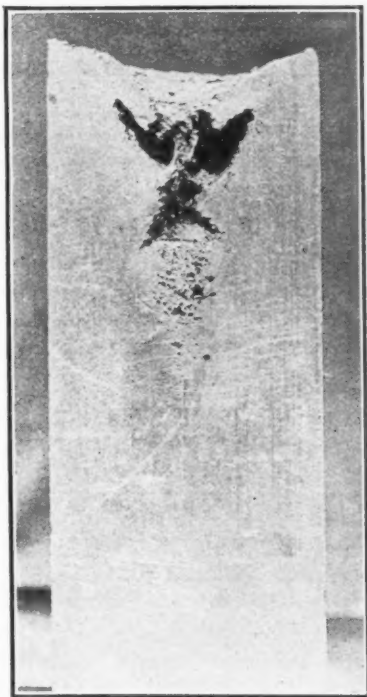


FIG. 3.—Section of an Improperly Cast Brass Billet.

tion increases the tensile strength, elastic limit and hardness, while reducing the elongation and reduction of area. Annealing removes the effect of rolling. This latter statement is subject to some modification in that the annealed cast bar has not quite the same physical qualities as after rolling and annealing. The effect of annealing is proportional to the temperature to which the metal is subjected. Characteristic curves showing the

effect of cold rolling and of annealing are given in Figs. 4 and 5. Different mixtures give widely different curves.

Before the material is taken to the finishing rolls, it is pickled in a weak sulfuric-acid solution, which removes the oxidation and discoloration due to the annealing process, after which it is rinsed in water. The action of the pickle is not com-

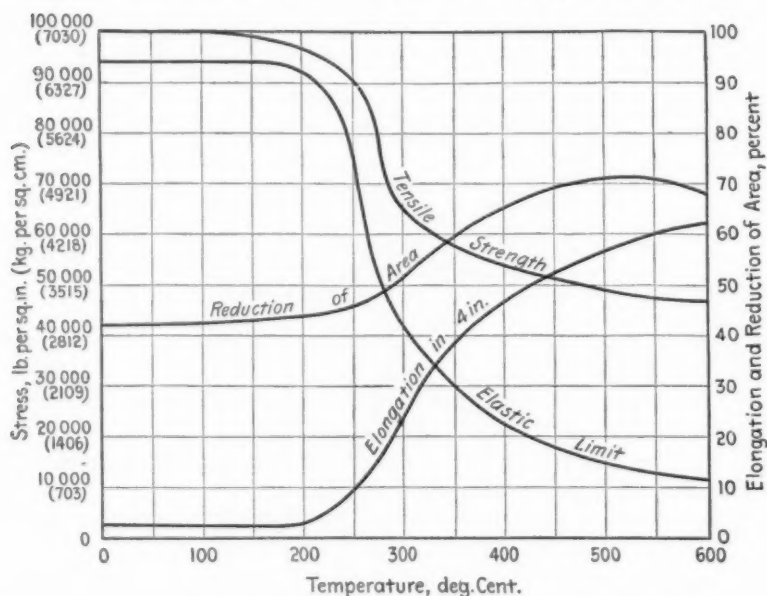


FIG. 4.—Curves showing Variation in Physical Properties due to Annealing Hard-Rolled Brass at Varying Temperatures.¹

plete in that certain irregular surface discolorations or stains frequently remain.

The final rolling process determines the finished thickness or gage and also the temper, both of which are subject to considerable variations from that desired. Strips of metal after passing through a rolling process will show two variations in thickness; one is a variation from side to side or across the width of the strip, the other from strip to strip or sometimes from one end

¹ At the time the tests were made from which these curves were plotted no accurate means of temperature measurement was available. The temperature scale is therefore presented as only approximate.

of the strip to the other. The former variation differs in extent for different widths, thicknesses and tempers, and cannot be entirely eliminated. It is, however, quite constant in any individual lot. The latter variation is due mainly to variations in hardness and thickness of the strip before passing through the rolls. This variation may be due to differences in composition or differences in degree of annealing. When a strip is passing through a set of rolls, the stress tending to separate the rolls is so great that a considerable amount of elastic deflection takes place not only in the rolls, but also in the housings. This deflec-

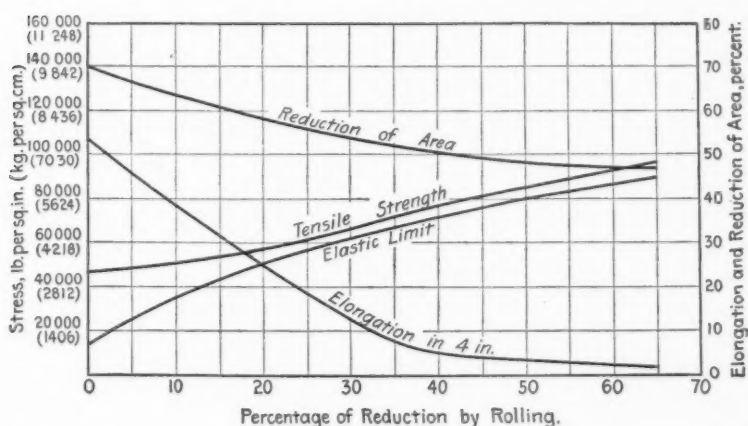


FIG. 5.—Curves showing Variation in Physical Properties of Rolled Sheet Brass, due to Varying Amounts of Cold Rolling.

tion is proportional to the stress, which is in turn proportional to the initial hardness and thickness of the metal.

It is the final rolling also which determines the temper of the material, this depending upon the amount of rolling given it after the final annealing. Fig. 4 indicates the wide range of properties due to variation in the amount of temper, and also shows the influences which the variation just discussed will have upon it.

A third quality due to the final rolling is flatness or freedom from buckles, more particularly in cases finishing with a temper. Subsequent flattening operations can more or less completely

remove such buckles. All of the above variations can be modified by skilful rolling but cannot be entirely prevented.

Following rolling, the material if required with a temper is ready for the finishing operations. If required to be soft, it is annealed, pickled, washed and dried out, the last operation being, as its name implies, for the purpose of quickly removing the rinsing water to prevent tarnishing. The strip is now ready to be cut to width and length. The former is done by passing the strip between rotary shears or slitters, which may merely remove the edges so as to produce a uniform width, or may cut it into a number of narrow strips. Because of the variations of thickness across the strip above referred to, the strips cut from the edges will always be thinner than those nearer the center. Another variation becomes apparent at this point, namely, a variation in longitudinal straightness. One cause of this is due to failure to roll the strip straight caused by unequal reduction of its two sides. In addition, when a strip is finished with a temper, even though it is straight as rolled, it may, if slit into a number of strips, become longitudinally curved due to unequal tension between the center and side elements. In the slitting operation, the strip is usually handled and shipped in a coil if not too thick, but may be required to be cut to some specific length and shipped flat. This usually produces a certain percentage of pieces of unequal length, which percentage increases inversely as the number of lengths which the original strip will make. If the net length of the strip is 20 feet and is required in 8-ft. lengths, two of the latter and one 4-ft. length will be produced.

TEMPER.

Alloys in sheet form will divide into two groups, those which are required soft or annealed and those having a temper. The former are usually for uses which involve distortion, producing flow of the material, and the qualities requisite for highest suitability vary somewhat with the method by which this flow is accomplished. Properties which permit the maximum amount of stretch will not necessarily equally admit of change of shape involving flow between supporting surfaces, as in drawing operations. Again, a condition permitting of maximum stretch due

to complete annealing may result in a roughening of the surface due to large crystal structure, which will materially interfere with subsequent polishing operations. With tempered brass, stiffness is the chief requisite, modified by ability to meet lesser amounts of distortion.

CLASSIFICATION OF KIND AND QUALITY.

Practice and trade usage have classified the copper-zinc alloys in sheet form into a number of groups according to the percentage of copper, the quality of spelter employed, and the added lead content. Several grades of temper are also recognized. The well-nigh universal test of quality, however, is the pragmatic test, the ability to meet the specific need of the user. These needs are arrived at by purely tentative methods and are satisfied by considering them in the light of experience. Attempts to quantitatively define needs, and to predict their satisfaction in terms of measurement, have not as yet been either numerous or highly successful.

DETERMINATION OF REJECTION LIMITS.

It is manifest that in such attempts difficulty will be encountered in establishing rejection limits, because of the number of variables governing the various factors. The possibility of concurrent variations in a number of cases demands quite wide limits. Moreover, since each crucible charge constitutes a heat or lot, consideration must be given to the fact that no method of obtaining a single sample representative of anything other than the crucible charge from which it is drawn is possible.

As previously stated, rods may be made by the extrusion process, by hot or cold rolling, and by drawing. The first three are usually roughing processes although all are extensively employed as final processes.

MANUFACTURE OF RODS AND BARS.

In the extrusion process, the cast billet is heated to redness and forced hot through a die. The resulting rod or bar may then require only to be cut to length, but is frequently subsequently drawn cold through a die in order to bring it more accurately

to dimensions, give it a temper and improve its finish. When the required section is relatively heavy each billet produces one continuous length; but with smaller sections, two or more are made simultaneously. This process is applicable only to those mixtures which are readily extrudable and is particularly effective in producing rods of irregular or difficult profiles.

More or less departure from specified cross-sectional dimensions occurs, which may be partially corrected by subsequent cold drawing. Variations in length must be permitted if the entire billet is to be utilized. The physical properties of the product of the extrusion machine are dependent upon the mixture employed, but vary somewhat with the temperature of the material at the time it passes through the die. Cold drawing modifies these properties similarly to cold rolling.

Extruded bars may be used for structural purposes, in which case the standard tension test furnishes a measure of the quality. The stress-deformation diagram is however of such a character as to make the location of the elastic limit or yield point a matter of some refinement. They may also be used for forging stock, in which case workability becomes a factor of importance. The process is also extensively employed for making rods for use in screw machines (turret lathes). For this purpose, the ability to cut freely is essential and is governed by the mixture employed. Accuracy of dimensions, straightness, soundness, homogeneity and freedom from imperfections are also required. The rod is extruded slightly larger than the required size, cold drawn, straightened, and cut to length.

Some mixtures cannot be either hot-rolled or readily extruded and must be cold-rolled or drawn. Usually the initial work is done by rolling and the final by drawing.

MANUFACTURE OF WIRE.

Wire is drawn from rods which may be either hot-rolled, cold-rolled, or extruded, and afterwards drawn through dies by means of rotating blocks until the finished size is reached. The effect of cold drawing is similar to cold rolling so that annealing is required at intervals. Small sizes of rod are produced by straightening wire.

Variations in the characteristics of wire are produced

precisely as in the case of sheet by variations in mixture, amount of hard drawing, and degree of annealing. No difficulty is experienced in keeping the diameter of wire within fairly close limits, as suitably made dies do not wear with great rapidity.

MANUFACTURE OF SEAMLESS TUBES.

Seamless tubes are made by cold-drawing from hollow cylindrical castings, by cold-cupping from circular blanks and by modifications of the Mannesmann hot-rolling process. The latter method is employed to produce a hollow blank which is reduced to size by cold drawing. There are numerous advantages and limitations possessed by the various methods. The primary requirement for a good tube is a sound casting. The most difficult factor to contend with is that of concentricity of the inside and outside surfaces. This is subject to relatively large variations even within the same length, although the inside and outside diameters can be closely controlled.

Seamless tubes are not usually employed for purposes requiring much distortion, that incident to bending being usually the most severe. Certain limitations to the degree of temper which it can possess, however, exist in a tendency possessed by hard tubes to crack spontaneously due to internal stress. Seamless tubes are used largely for purposes involving exposure to the action of corrosive agencies such as sea water, and under such circumstances exhibit the erratic behavior shown by other materials under like exposure. Much difference of opinion exists as to the ability to resist corrosion, or lack of it, imparted by variations in percentages of copper and zinc, and other ingredients usually accompanying these, as well as to the results of various manufacturing methods and processes.

THE RELATION OF QUALITIES TO USES.

From the considerations above discussed, it will be seen that the problem of preparing rational specifications for wrought non-ferrous materials in various forms, and for various uses, is one involving many complex factors. It requires a comprehensive knowledge expressed in terms susceptible of definition and measurement of the qualities, which for any particular use are, on the one hand, necessary or desirable and, on the other

hand, attainable. For example, it is possible to limit the presence of lead and iron always found in brass to quite low limits, and for certain purposes such limits are quite essential. There exists, however, a wide variety of uses which, to be most efficiently met, demand the presence of lead in varying amounts, while there are still other uses wherein it is neither beneficial nor harmful. Similarly, iron in considerable amounts may be advantageous, detrimental, or merely innocuous, according to the employment contemplated.

THE CONTROL OF QUALITY.

The difficulty of properly controlling the quality of material made in such small lots as to render impracticable the employment of suitable tests, physical or chemical, for each lot is however largely surmountable by the employment of appropriate mill practice and inspection. The test of manufacture is, in the case of cold-rolled or drawn material, capable of being made most severe. It has the exceedingly great advantage of application to an entire quantity, and not merely to a sample. No test of ductility, for example, can be more complete than that of drawing through a die, when the draft is properly chosen with respect to the ultimate ductility of the material operated upon.

DISCUSSION.

MR. G. H. CLAMER.—Mr. Webster has shown some tests Mr. Clamer.
here of brass at elevated temperatures. A very interesting paper was read at the March meeting of the British Institute of Metals, showing the effect of high temperatures on bronze mixtures which contained lead, in which the curious fact was noted that, at elevated temperatures, the alloys containing lead gave higher values, both in tensile strength and elongation, than the alloys without lead. I should like to ask Mr. Webster if he has noted any effect of lead or other metals on brass, that is, copper and zinc alloys, at elevated temperatures, as compared with brass without lead or other metals.

MR. W. REUBEN WEBSTER.—Mr. Clamer has misunder- Mr. Webster.
stood these curves, in that the tests they record were all made at atmospheric temperatures. The material was heated to the various temperatures, withdrawn from the furnace and allowed to cool, so that the effect of lead or other ingredients at high temperatures would not be shown by these curves. I would say that I have never observed that characteristic. We have made quite a number of tests on various materials at various temperatures; certain of the brass group are weak and brittle at high temperatures, and certain others, marked by the fact that they can be hot-rolled, are quite ductile when red hot. Then there are other ranges of temperature over which these materials are more or less brittle.

MR. T. D. LYNCH.—I should like to ask if the element of Mr. Lynch.
time was taken into consideration in these tests.

My experience has been that there is a very appreciable effect under relatively low temperatures when applied to brass or copper for long periods of time, whereas a much higher temperature is necessary to produce the same results when applied for short periods of time. It would therefore be interesting to learn whether Mr. Webster considered both time and temperature as elements in his tests.

I wish to say also that I am very glad to have had the privilege of hearing this paper presented before the Society, especially

Mr. Lynch. by a man who is at the front rank in the manufacture of this line of material. It gives us information that is, I am sure, very valuable.

Mr. Webster. **MR. WEBSTER.**—The time element was not taken into consideration here, because the time employed was the time ordinarily used in the annealing process in the ordinary course of operations in brass manufacture. The time element is not very important in the mill; it is the temperature that is most important. When you come to annealing things for weeks, as the physicists do, time is a different matter, but practically all annealing operations in the mill take place within one hour. There are certain classes of work where it is necessary to anneal for three or four hours, but in the ordinary run of work in the copper and zinc group, time is not an important factor.

Mr. Capp. **MR. J. A. CAPP.**—I can easily understand that in the process of annealing sheet metals time does not ordinarily enter as a determining factor, yet the user of the sheet or strip metal for drawing or cupping operations does take advantage of the influence of time in annealing. In our own practice, there are cases where we are forced to make use of the time as an element. In one case the production is on such a scale and in such a wide variety of shapes that the only practical way is to run the annealing furnace at a fixed temperature. The drawn parts are then annealed by holding them in the furnace for longer or shorter times, according to a schedule developed by practical experience. In another department it is found more advantageous to fix rigidly the time the parts are in the furnace, and vary the temperature of the furnace. The annealing done is only partial annealing. I believe the mill man would be apt to speak of it as "light annealing," or "full annealing," or by some equivalent term. The necessity for such annealing arises from the fact that in the drawing or cupping operations, the draft or movement of the metal required is such that its ductility is not sufficient to permit of its being done at one operation without breaking. Therefore the operation is done in stages with a partial restoration of ductility by partial annealing between operations. The object is to have a metal with just sufficient ductility to permit formation of the part desired, and yet have this part as stiff as possible when completed.

I am very glad to have heard this paper and to know that

it will appear in our Proceedings, because it nicely indicates the difficulties those of us who have had to do with the purchasing of brass for drawing operations have had in trying to tell the brass mills just what we want. We do not know how to express in measurable terms the needed qualities of ductility, stiffness, and the like, and the mills do not know how to test the materials so that they can determine when they have given us the right quality. The only thing possible, then, is a "cut-and-try" process, in which samples of varying temper are submitted for trial, and the right quality and temper worked out. The mill is given all the information possible at the start, and they then, based on experience, supply the samples of material which they believe will be approximately right. We know of no other way of dealing with this matter, the only practical specifications being that the brass must be suitable for the purposes intended.

MR. C. O. HARRINGTON, JR.—I should like to ask Mr. Webster whether the curves shown in Fig. 4 are to be taken as applying to phosphor-bronze. We use phosphor-bronze where it is necessary for us to heat-treat it at a very low temperature, under 150° C. for a long period, possibly a hundred hours. We do this in order to enable the metal, usually in the form of springs, to attain a permanent set after we have bent or deformed it. Would there be any danger of phosphor-bronze losing its elasticity in a marked degree by that long heat treatment at a low temperature? According to the curves just presented brass would not be affected that way. Is there any possibility of phosphor-bronze being affected by continuous heat treatment of about a hundred hours at less than 150° C.?

MR. WEBSTER.—I have never had occasion to make any experiments on extended annealing, so I am not able to say anything about that. I have made some extensive experiments on short-time annealing by annealing brass and various other materials in a lead pot where the desired temperature is attained in a very short time, and comparing the results obtained in that way with the results obtained in the more normal period. Phosphor-bronze and, in fact, all of the ductile materials, have the same general characteristics as the curves shown here, but the values are different for different materials. For instance, the breaking-down point occurs at different temperatures for

Mr. Webster. different materials, and the maximum degree of reduction of area is attained at different temperatures for different materials. There are some materials which do not attain nearly as great a percentage of elongation as others. For instance, you can anneal copper as long as you please and at as high temperature as you please, but you will not get much over 45 per cent elongation.

Answering Mr. Capp, I wonder if the material he refers to necessarily attains the same temperature in each case; that is, does the material which has been annealed for five minutes attain the same temperature as that which has been annealed for half an hour? It is a common practice to put material in a high-temperature furnace and give it the right anneal by leaving it in there only until it attains a certain temperature and then withdrawing it. The material does not always attain the same temperature as the furnace, for that takes time. It is necessary to know the temperature of the material rather than that of the furnace.

Mr. Capp.

MR. CAPP.—The furnace I spoke of as one in which we allow the metal to remain for different times, is always of much higher temperature than that we desire to have the metal reach. The drawn parts are spread out in rather thin layers, placed in the furnace and the door closed for a given period, when the furnace is re-opened and the parts taken out quickly. Whether they always reach exactly the same temperature, it is impossible to determine, but by "cut-and-try" methods a schedule of times has been worked out which gives the best average results.

Mr. Webster.

MR. WEBSTER.—It was to settle that exact point that I made the experiments on annealing in a lead pot. Apparently, the samples attain the temperature of the lead pot in a very short time indeed. It is a matter of a small number of seconds, and we have found that the temperature is the important element and not the time. When you put the material in a high-temperature furnace and pull it out at various periods, you simply pull it out when it has attained a certain temperature. Ordinarily the physical characteristics of that material will be those normal to that temperature, that is, the temperature the material attains at the time it is pulled out of the furnace. The practice Mr. Capp speaks of is common and is known to every worker of brass.

STUDY OF THE STRENGTH OF NON-FERROUS CASTINGS: COMPARISON OF DIFFERENT TEST SPECIMENS.

BY L. P. WEBBERT.

SUMMARY.

In this paper the author describes two types of test specimens used on non-ferrous castings; one type used on castings made of copper-tin alloys and the other type used on castings made of copper-zinc alloys. The results obtained from the former type are affected by the diameter of the test specimen as cast and by the amount of metal turned off to finish the specimen. The results obtained from the latter type are not affected to any such degree with change in the thickness of the coupon as cast and by the amount of metal turned off.

Both types of test specimens were cast on blocks of metal, in order to study their relative advantages when the castings were made of copper-tin and copper-zinc alloys. A test specimen was cut from the center of each block of metal. The analysis of the alloys and the tensile strength and percentage of elongation of each test specimen are given in Tables I and II. The author desires to direct attention to the fact that the decrease in strength from that given by the outer test specimen to that given by the test specimen at the center of a block of metal is more decided in a copper-tin casting than in a similar copper-zinc casting.

A brief description of the manufacture of the alloys is given. These alloys are used in engineering practice.

The author concludes that the test specimen used for copper-zinc castings represents very closely the strength of the casting as a whole, but that the test specimen used for copper-tin castings does not represent the strength of the casting as a whole and gives high values.

STUDY OF THE STRENGTH OF NON-FERROUS CASTINGS: COMPARISON OF DIFFERENT TEST SPECIMENS.

BY L. P. WEBBERT.

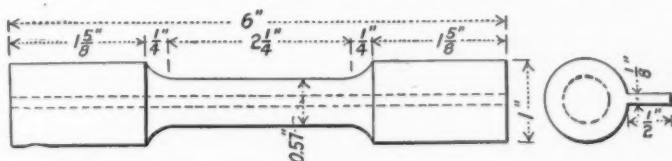
The object of the investigations described in this paper is to note the changes in strength that occur in different thicknesses of metal castings made from the non-ferrous metals met with in engineering practice. The knowledge desired, from the machine-designer's point of view, is to know how to proportion the separate parts of a design with regard to strength, and to know what physical properties to expect from a casting at different points or as a whole.

DESCRIPTION OF TEST SPECIMENS.

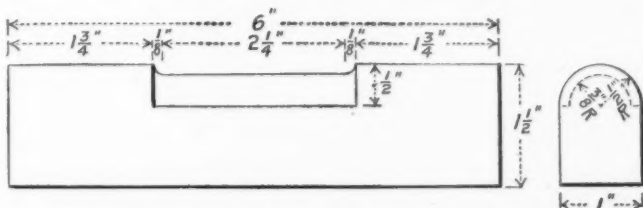
Some metallurgists strive to adopt a test specimen to represent the strength of an alloy which will give the maximum results obtainable for that metal. Such test specimens give results that are interesting but tend to mislead. Quite a number of different types have been suggested for adoption as a standard test specimen, but when we consider the specifications that must be followed in practice our choice becomes decidedly restricted to a few forms. The specifications state that a test specimen is to be a part of the casting and is not to be cast separately. The test specimen must receive the same heat treatment—that is, no chilling of the test specimen is allowable—and must be turned down to a stated diameter.

There are two styles of test specimens made use of in practice, one for the copper-tin alloys and the other for the copper-zinc alloys. The first is that used on 88-10-2 castings (copper 88 per cent, tin 10 per cent, zinc 2 per cent). This test specimen, as cast with the casting, has the dimensions shown in Fig. 1 (a). A $\frac{1}{8}$ -in. gate running the entire length of the test specimen connects it with the casting. A casting with the test specimen attached is shown in Fig. 2. Being gated in this manner allows the test specimen to be filled quickly with metal and has the advantage of giving the same rate of cooling to every

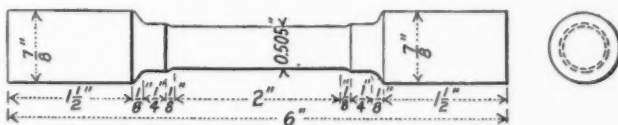
part of the specimen. When the test specimen is detached it is turned down in a lathe to the proper diameter and form as shown in Fig. 1 (c). In Fig. 4 (c) are shown the patterns placed in the parting of the mold; two patterns to give test specimens 0.505 in. in diameter, and one to give a test specimen 0.798 in. in diameter. When a test specimen is to be placed at another part of the casting, cores are made use of as shown in Fig. 5 (b)



(a) Test Specimen for 88-10-2 Casting, as Cast.



(b) Test Specimen for Copper-Zinc Alloys, as Cast.



(c) Test Specimens (a) and (b) as Finished.

FIG. 1.

and (c). At (b) are shown cores to give 0.798-in. test specimens and at (c) are shown those to give 0.505-in. test specimens.

The test specimens as shown in Fig. 1, with finished diameters of 0.505 in., are used more than test specimens having finished diameters of 0.798 in. A drawing of the 0.798-in. specimen would be similar to those in Fig. 1, and would show larger diameters and greater thickness of gate.

For the copper-zinc alloys (manganese-bronze and brass) a bar of metal 1 by 1 1/2 by 6 in. is cast on the side of the casting. This test specimen has the dimensions shown in Fig. 1 (b).

Fig. 4 (b) shows two patterns for this style of test specimen, the diameter of which is to be 0.505 in. These patterns are placed against flat surfaces. For curved surfaces different patterns are made to fit the curvature of the castings. The round part of the pattern shown in Fig. 4 (b) allows the pattern to be drawn easily from the sand. Comments on this style of test specimen will be made again in this paper.

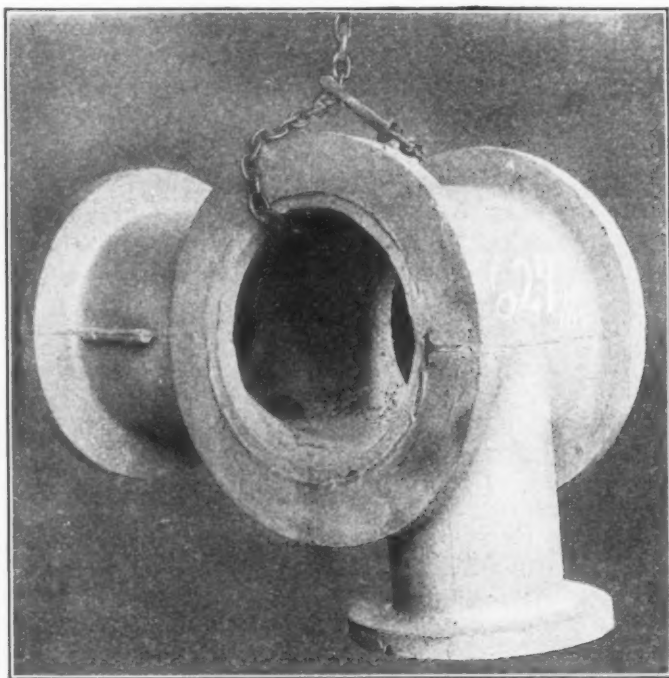


FIG. 2.—88-10-2 Casting showing Test Specimen Attached.

It is well known that to obtain the highest results from 88-10-2 test specimens the least amount of metal should be turned off the specimen. In comparing Fig. 1 (a) with (c) it will be noted that 0.065 in. of metal is turned from the test specimen. In the "skin" of the casting lies the greatest strength, as the foundryman puts it. To illustrate: A large valve body, $\frac{3}{4}$ in. in thickness, made from 88-10-2 metal, was cut up to procure test specimens from the flange and body of the casting,

in order to compare the strength of these with the strength of the test specimen attached to the casting. The test specimen (0.505 in. in diameter) gave an ultimate tensile strength of 40,000 lb. per sq. in. and an elongation in 2 in. of 40 per cent. From the body the following results were obtained:

	No. 1.	No. 2.
Tensile strength, lb. per sq. in.	25 800	25 200
Elongation in 2 in., per cent.	13.0	11.0

From the flange the following results were obtained:

	No. 1.	No. 2.
Tensile strength, lb. per sq. in.	23 650	18 850
Elongation in 2 in., per cent.	11.0	5.0

When the test specimen was turned down to the proper diameter, very little of the skin was turned off. The test pieces from the interior of the metal possessed no skin and gave lower results. The results emphasize the fact that the test specimen attached to the casting exaggerates the strength of the casting as a whole.

If the diameter of this style of test specimen is increased lower results are obtained. Let us compare the 0.505-in.-diameter test specimen (0.2 sq. in. in area) with the 0.798-in.-diameter specimen (0.5 sq. in. in area). The ratio of the area of the 0.505-in. test specimen to the circumference is smaller ($\text{area} \div \text{circumference} = 1.26$) than the ratio of the area of the 0.798-in. test specimen to its circumference ($\text{area} \div \text{circumference} = 1.99$). In the former case we state that there is more skin, so to speak, to the specimen and consequently more strength obtainable than with the larger-diameter specimen. The author has averaged 61 tests from large castings having the 0.505-in. test specimen and gives the following average: Tensile strength, 46,580 lb. per sq. in.; elongation in 2 in., 38.7 per cent. Among these tests a maximum tensile strength of 51,650 lb. per sq. in. is noted, with an elongation of 62.5 per cent, which is also the maximum elongation. The minimum tensile strength among the tests is 39,100 lb. per sq. in. with an elongation of 25 per cent. The minimum elongation is 20 per cent. The specifications under which these castings were made required a minimum tensile strength of 30,000 lb. per sq. in.

and a minimum elongation of 15 per cent in 2 in. A specification from another source called for a minimum tensile strength of 35,000 lb. per sq. in. and a minimum elongation of 15 per cent in 2 in. for a test specimen of 0.5 sq. in. area (0.798 in. in diameter). This style of test specimen was placed on ten large 88-10-2 worm wheels. The tests gave an average tensile strength of 36,324 lb. per sq. in., and an average elongation of 27.1 per cent. The maximum tensile strength among the tests was 42,800 lb. per sq. in., with an elongation of 36 per cent, which was also the maximum elongation. The minimum tensile strength was 29,300 lb. per sq. in., with an elongation of 17 per cent, which was the minimum elongation. Among these ten wheels there were two—one with a test specimen that pulled 32,500 lb. per sq. in. with an elongation of 21 per cent, and another with a tensile strength of 29,300 lb. per sq. in., and an elongation of 17 per cent—that did not pass specifications. No doubt exists that had the 0.505-in.-diameter test specimen been used all tests would have given higher results.

We can conclude, therefore, that for this style of specimen used on copper-tin alloys, the smaller ratio of area to circumference of test specimen gives the greater strength. This is due to the fact that in general the rate of freezing of the metal is an important factor in influencing the strength of the casting. The skin of the casting receives the chill of the mold, is the first to freeze, and shows the smallest structure or grain. From the skin, freezing of the metal takes place gradually toward the center of the casting. The rate of freezing depends upon the following factors: (1) The thickness of the casting; (2) the temperature of the metal entering the mold; and (3) the conductivity of the mold.

VARIATION OF THE STRENGTH OF METAL AT DIFFERENT PARTS OF A CASTING.

In order to study the effect of the rate of freezing upon the strength of the metal, castings of copper-tin and copper-zinc alloys were made as shown in Figs. 3, 4 (*a*) and 5 (*a*). Fig. 5 (*a*) shows a block made of 88-10-2 metal with the test specimens attached which are used for this metal. Fig. 3 is inserted to show the arrangement of specimens and number-

ing on blocks made of copper-tin and copper-zinc alloys. Test specimens Nos. 1, 2 and 4 are of the pattern shown in Fig. 1 (a). No. 3 is a similar test specimen to be turned down to a diameter of 0.798 in. Nos. 5, 7 and 8 are test specimens used on castings made of copper-zinc alloys (manganese-bronze and brass). A bar of metal is cut out of the center of the casting and turned down into a test specimen to represent the strength of the metal at the center. The relative advantages of these types of test specimens can be studied, since they are cast together on the same casting, are made from the same metal, and receive the same heat treatment. The specimens

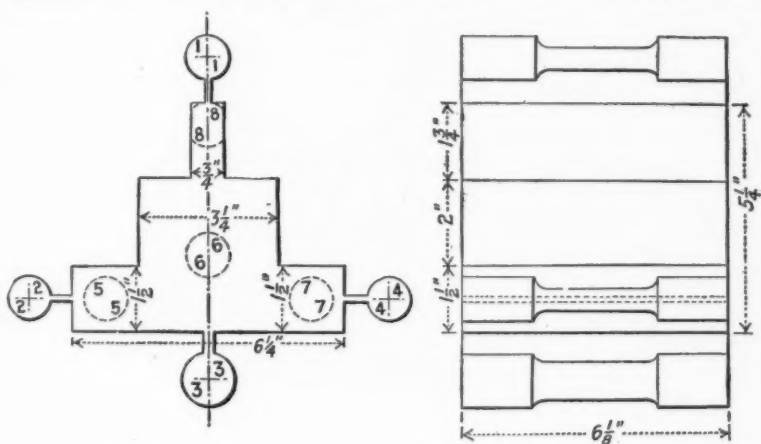


FIG. 3.—Test Block showing Dimensions and Numbering of Test Specimens.

on all blocks made were numbered as shown in Fig. 3. Tables I and II give the results of all tests. The test specimens were not threaded but were pulled with grip jaws in the testing machine. In turning down the test specimens, Nos. 5, 6 and 7 were given a diameter of 0.798 in. The diameter was later changed to 0.505 in. in order to obtain uniformity in the size of the test specimens. Test specimen No. 3 was given a diameter of 0.798 in. for all blocks. In some instances this specimen was turned down to a smaller diameter when found slightly defective.

A few changes had to be made when casting the manganese-

bronze blocks, on account of the nature of the metal. The risers were made larger, the sprues longer and the gates connecting test specimens Nos. 1, 2, 3 and 4 were made thicker. On the brass blocks the sprues were made similar to those of the manganese-bronze blocks; otherwise they were similar to those of the copper-tin alloys. After casting, the metal was churned in the riser in every case. Specimens Nos. 1, 2 and 4 were attached with the intention of observing how closely they would check each other when tested, thereby testing the reliability of this type of test specimen when made of the different metals. It may be men-



FIG. 4.—(a) Manganese-Bronze Test Block.
 (b) Manganese-Bronze Test Specimens.
 (c) Test Specimens for Copper-Tin Alloys.

tioned that this type can not be depended upon when attached to brass castings.

With some alloys a green-sand mold was made. This mold was filled immediately after the dry-sand mold was filled. The metal for both molds was poured from the same crucible. The castings made in green sand gave results that showed no decided variation from those made in the dry sand. The green-sand molds were soon discontinued. Only the average results from the green-sand blocks are given in Table II.

The block for alloy No. 1 was cast after the casting shown

in Fig. 2 was poured. This metal was made from the best copper, tin and zinc obtainable, but was not remelted. The temperature was not taken when the block was cast but was lower than the pouring temperature usually desired for such a small casting. The specimen attached to the casting in Fig. 2 was pulled and gave a tensile strength of 49,150 lb. per sq. in. and an elongation of 47.5 per cent.

For alloy No. 2 a different procedure was adopted. In order to get a perfect and homogeneous alloy the metals for this block were alloyed and the resulting alloy pigged. The metal was remelted, brought to the proper temperature in the

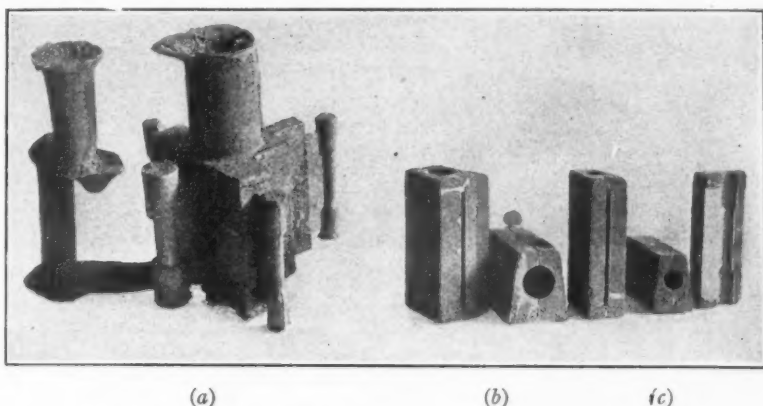


FIG. 5.—(a) 88-10-2 Test Block.

(b) and (c) Cores used for Specimens Nos. 1, 2, 3 and 4, Fig. 3.

furnace and cast. The casting after being cut up was found to be perfect.

Alloy No. 8, manganese-bronze, gave trouble. At the center of the block near the top sponginess occurred, due to shrinkage. In practice this trouble is overcome by pouring metal at a higher temperature from another crucible into the riser during churning to liven up the metal in the center. Only one attempt was made to make a block of manganese-bronze pulling about 80,000 lb. per sq. in. This casting was a failure due to the little shrink holes occurring throughout the casting. One test was good, No. 1, which gave a tensile strength of 79,500 lb. per sq. in. and an elongation of 24 per cent.

The block for alloy No. 9 was not finished and is inserted in Table II because the analysis was made.

Methods of Manufacture.—The alloys used were made by melting the copper under charcoal, then adding the zinc, tin and lead in the order named. The temperature of the melted copper was allowed to become sufficiently high to cause the zinc to flare when added in the case of those alloys requiring a large amount of zinc. To the alloys high in tin, 1 oz. of 15-per-cent phosphor-copper was added for every 100 lb. of metal after the other metals had been alloyed, the object being to introduce sufficient phosphorus to remove the oxides and leave a trace of phosphorus in the alloy.

TABLE I.—ANALYSES OF ALLOYS.

Alloy No.	Percentage of							
	Copper.	Tin.	Zinc.	Lead.	Iron.	Manganese.	Aluminum.	Phosphorus.
1	88.31	8.49	3.20	trace
2	87.98	10.64	1.28	0.10	trace
3	86.78	6.31	5.96	0.92	0.03	trace
4	84.28	11.31	4.15	0.06	0.06	trace
5	62.81	0.79	36.18	0.09	0.05	0.02	0.06
6 ^a	62.81	0.79	36.18	0.09	0.05	0.02	0.06
7	84.49	15.49	0.02
8 ^b
9	57.66	0.84	39.85	0.69	0.16	0.58
10	89.08	7.48	3.00	0.07	0.20

^a Alloy No. 5 remelted. Analysis not made.

^b Analysis not made. Manganese-bronze.

Manganese-bronze was made by introducing an alloy of iron, manganese and tin into the melted copper when sufficiently hot. This alloy, which is made by melting together iron and ferro-manganese, has an approximate composition of 54 per cent iron, 21 per cent manganese and 25 per cent tin. When this iron and ferro-manganese are melted the tin is introduced to lower the melting point. The amount of this alloy introduced depends upon the percentage of iron desired in the bronze. Tin is added later with the zinc to keep the percentage of tin at 0.8. After the above alloy had dissolved in the copper the aluminum was added and then the zinc and tin. The manganese-bronze was pigged before being used again.

In taking pyrometer readings no attempt was made to get

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TABLE II.—RESULTS OF TESTS.

ALLOY NO. 1.—COPPER-TIN; NEW METAL; NOT REMELTED.

Test Specimen No.	Diameter of Test Specimen, in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.
1.....	0.505	40 800	28.0
2.....	0.505	43 250	32.0
3 (not cast).....	0.798	35 720	24.5
4.....	0.505	33 750	22.5
5.....	0.798	33 080	21.0
6 ^a	0.505	34 600	24.0
7.....			
8.....			
Average.....		36 867	25.3

^a Specimen broke in lathe.

ALLOY NO. 2.—COPPER-TIN; NEW METAL; PIGGED AND REMELTED.

Temperature of Metal, 2300° F.

1.....	0.505	51 250	42.5
2.....	0.505	50 300	40.5
3.....	0.798	40 660	23.5
4.....	0.505	50 400	39.0
5.....	0.798	33 740	14.0
6.....	0.798	28 560	9.5
7.....	0.798	34 200	16.0
8.....	0.505	40 550	28.0
Average.....		41 208	26.6

ALLOY NO. 3.—COPPER-TIN; NEW METAL; NOT REMELTED.

Temperature of Metal, 2100° F.

1.....	0.505	44 150	42.0
2.....	0.505	38 250	30.0
3.....	0.798	36 460	27.0
4.....	0.505	40 350	31.0
5.....	0.505	36 600	37.0
6.....	0.505	37 200	30.0
7.....	0.505	27 050	17.5
8.....	0.505	39 100	40.0
Average.....		37 395	31.8
Green-sand mold, average.....		35 190	30.5

ALLOY NO. 4.—COPPER-TIN; NEW METAL; NOT REMELTED.

Temperature of Metal, 2200° F.

1 ^b	0.505	27 200	2.5
2.....	0.505	42 500	15.0
3.....	0.798	32 040	6.0
4.....	0.505	39 700	11.0
5.....	0.505	34 250	11.0
6.....	0.505	20 700	1.5
7.....	0.505	32 250	7.5
8.....	0.505	37 750	14.0
Average.....		34 170	9.4
Green-sand mold, average.....		33 502	11.0

^b Defective test; not included in averages.

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TABLE II (Continued).—RESULTS OF TESTS.

ALLOY NO. 5.—COPPER-ZINC; NEW METAL; NOT REMELTED.
Temperature of Metal, 1800° F.

Test Specimen No.	Diameter of Test Specimen, in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.
1.....	0.505	39 450	34.0
2.....	0.505	38 000	29.0
3.....	0.505 ^a	43 180	42.0
4.....	0.505	34 300	25.5
5.....	0.505	41 950	46.0
6.....	0.505	41 450	40.0
7.....	0.505	42 750	47.0
8.....	0.505	43 300	49.0
Average.....		40 548	39.1

^a Defective for diameter = 0.798 in.

ALLOY NO. 6.—COPPER-ZINC; ALLOY NO. 5 REMELTED.
Temperature of Metal, 1840° F.

1.....	0.505	49 250	40.0
2.....	0.505	44 000	25.0
3.....	0.798	48 200	37.0
4 ^b
5.....	0.505	50 000	48.0
6.....	0.505	49 250	47.0
7.....	0.505	49 800	45.0
8.....	0.505	50 000	46.0
Average.....		48 643	41.1
Green-sand mold, average.....		48 421	35.0

^b Defective test.

ALLOY NO. 7.—COPPER-ZINC; NEW METAL; NOT REMELTED.
Temperature of Metal, 2100° F.

1.....	0.505	31 450	39.5
2.....	0.505	30 850	44.0
3.....	0.798	31 400	37.0
4.....	0.505	32 200	52.0
5.....	0.505	29 000	52.0
6 ^c
7.....	0.505	28 900	55.5
8.....	0.505	31 700	44.0
Average.....		30 786	46.3
Green-sand mold, average.....		29 435	47.0

^c Defective test.

ALLOY NO. 8.—COPPER-ZINC; NEW METAL; PIGGED AND REMELTED.
Temperature of Metal, 1800° F.

1.....	0.505	69 150	32.5
2 (not cast).....
3.....	0.698 ^d	69 990	29.0
4 (not cast).....
5.....	0.798	68 880	29.0
6.....	0.798	31 780	1.5 ^e
7.....	0.798	68 740	29.0
8.....	0.505	67 850	25.0
Average.....		68 922	28.9

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TABLE II (*Continued*).—RESULTS OF TESTS.

ALLOY NO. 9.—COPPER-ZINC; NEW METAL; PIGGED AND REMELTED.

Test Specimen No.	Diameter of Test Specimen, in.	Tensile Strength, lb. per sq. in.	Elongation in 2 in., per cent.
1.....	0.447	73 885	26.0
2.....	0.505	73 200	26.0
3.....	0.798	73 100	24.0
4.....	0.798	72 800	26.0

ALLOY NO. 10.—COPPER-TIN; NEW METAL; NOT REMELTED.

Temperature of Metal, 2100° F.

1.....	0.505	33 000	16.0
2.....	0.505	40 600	28.5
3.....	0.798	31 480	16.0
4.....	0.505	28 950	10.0
5.....	0.505	25 750	12.5
6.....	0.505	27 150	16.0
7.....	0.505	20 600	9.0
8.....	0.505	40 150	48.0
Average.....		30 960	19.5
Green-sand mold, average.....		32 070	20.2

the casting temperature at the mold, because the pyrometer in use was a base-metal couple that was not sufficiently sensitive to give quick accurate readings; also because pyrometer readings are best made at the furnace, on account of the necessity of removing the metal from the furnace when the proper temperature has been reached. This procedure does not allow the metal to overheat or "soak" (that is, to remain in the furnace an unnecessary length of time), which injures the alloy. In Table II the temperature given is the temperature taken at the furnace.

CONCLUSION.

On comparing the different tests we can conclude that the 88-10-2 test specimen as shown in Fig. 1 is convenient to use on copper-tin alloys. The 88-10-2 specimen with its narrow gate was attached to a block of manganese-bronze with no success. The gate was then widened to the thickness of the test specimen at the middle as used on the blocks for alloys Nos. 8 and 9. The adopted test specimen for manganese-bronze (0.505 in. diameter) took the form shown in Figs. 1 (b) and 4 (b). The 88-10-2 specimen was not successful with brass due to small cracks forming in the specimen. These small cracks were

not noticeable until the specimen was tested. The rate of freezing appears not to affect the copper-zinc alloys to as great an extent as the copper-tin alloys. The strength of alloy No. 2 varied from a tensile strength of 51,250 to 28,560 lb. per sq. in., while that of alloy No. 7 varied from a strength of 31,700 to 29,000 lb. per sq. in. With this copper-zinc alloy no cracks developed in the test specimens Nos. 1, 2, 3 and 4.

Considering a test specimen to represent the strength of a copper-zinc alloy, we can choose such a specimen as Nos. 5 or 6 for a diameter of 0.798 in., or No. 8 for a diameter of 0.505 in. (the specimen shown in Figs. 1 (b) and 4 (b) is identical with No. 8). But when the copper-tin alloys are considered we can not make a choice. In a gear blank made from 88-10-2 metal, the surface of the metal is cut away to form the teeth of the gear. It is evident that the strength of the teeth could not be represented by test specimens Nos. 1, 2, 3 and 4. If a bar of the same metal is turned down to the size of a bolt, the designer's calculations must be based on the strength of the metal represented by specimens Nos. 5 and 7.

In this paper the author has endeavored to point out, with a limited number of tests, how the strength of metal varies with changes in the thickness of castings, what strength to expect from different alloys, and how a study was made of the different types of test specimens used in practice.

DISCUSSION.

MR. H. E. SMITH.—I should like to ask if any experiments **Mr. Smith.** were made on compression test pieces? Some engineers specify the compression test for certain castings, for example, bearings for turntables, and I have seen some very unfortunate test pieces used for that purpose.

MR. L. P. WEBBERT.—I have made no compression tests **Mr. Webbert.** on metals but I have made shear tests. In referring to Fig. 3, the specimen No. 5 was turned down and pulled. I assumed from the strength of specimen No. 5 that the strength of specimen No. 7 would be the same. I have made experiments on other blocks, pulling both specimens and getting the same results. I wanted to establish the relations between shear and tensile strength. Of course had I taken the tensile strength of a No. 1 specimen and compared it with the shearing strength of No. 7 specimen, the result would not have been satisfactory.

MR. W. M. CORSE.—I should like to ask Mr. Webbert, if **Mr. Corse.** the difference in the copper-tin series, between the actual strength of the casting and the strength of the test bar, would indicate that the quicker chilling gives the greater strength? Would it not be a good thing to go the limit, as it were, on this chilling and cast test bars in chills, when you are comparing various alloys and not castings? When you are comparing two alloys and not two different castings, it seems to me that if the chill effect, as shown by using cast-to-size bars, is too marked, it might be better to use all-chill bars and from them get properties which would allow you to compare the alloys, rather than to use any of the test bars that you have enumerated. What do you think of that scheme?

MR. WEBBERT.—I think that is all right, but the object in **Mr. Webbert.** view in writing this paper was to show the designer of machinery the error made in using the results obtained from the test bars. There is a temptation to take the results of the test specimen and figure the strength of the whole casting or whatever part of

Mr. Webbert. machinery he is designing. But I think the chill would be a satisfactory method of comparing the properties of the alloys.

Mr. Corse. MR. CORSE.—More particularly in the copper-tin series?

Mr. Webbert. MR. WEBBERT.—Yes, sir.

Mr. Corse. MR. CORSE.—Is there not also the temptation for the manufacturer to publish the high results from his test bars in order to influence business? This may get him into trouble, because, when a man does cut a piece out of his casting, he may find a low strength. Calling the engineer's attention to the fact that his casting does not test what his test bar usually shows is, I think, one of the best things that the brass foundryman can do.

Mr. Macgregor. MR. J. S. MACGREGOR.—I recently have had brought to my attention, as a result of testing several manganese-bronze bars, and also upon looking up the tests of other individuals, the fact that manganese-bronze poured in a chill mold will be more ductile and have greater strength than when not so poured, which is quite the reverse of the effect one would naturally expect.

Mr. Clamer. MR. G. H. CLAMER.—This paper of Mr. Webbert is exceedingly interesting in connection with the work of Sub-Committee III, on Sand Cast Metals and Alloys, of Committee B-2. We have recommended a specification for manganese-bronze ingots, but we have not yet dared to offer a specification for manganese-bronze castings, as we were immediately confronted with the question of a standard test bar. In order to arrive at some conclusions as to a satisfactory test bar, the Society now, as Mr. Corse told you at the beginning of the meeting, together with the Institute of Metals and the Institute of Mining Engineers, is cooperating with the Bureau of Standards.¹ The Bureau of Standards has appropriated a sum of money and has had a man delegated for that work. He has been on it for a year or more, and has made a vast number of tests, which have yielded very good data and information. Probably after a little further study of the subject we shall be able to arrive at some satisfactory test bar for non-ferrous metals, but it is quite certain that one form of test bar for all the non-ferrous metals cannot be used. Mr. Webbert has shown that the test bar is apparently satisfactory for the copper-zinc alloys, whereas for the copper-tin

¹ *Proceedings*, Am. Soc. Test. Mats., Vol. XIV, Part I, pp. 181-182 (1914).—Ed.

alloys it is not. The test bar, or rather his method of attaching it to the casting, is quite ingenious, and I think it is something which our committee should carefully consider: that is, the attaching of a cast-to-size bar with the long thin gate, thus feeding the test bar over practically its entire length. I think in that way the test bar can be properly fed and that the results from such a test bar would be entirely different from the results obtained were the same test bar attached at right angles to the casting, and the metal allowed to flow in from one end. Mr. Lynch has had some interesting experience with manganese-bronze in his every-day foundry practice. He uses a hollow drill and bores right through the casting. Probably he can tell us just how such a test bar compares with the test bar that is attached to the casting. I should like to hear from him on that subject.

MR. T. D. LYNCH.—It is our practice to hollow-drill test samples from the main body of each manganese-bronze casting. We find it practicable to obtain a tensile strength of 60,000 lb. per sq. in., a yield point 30,000 lb. per sq. in., and an elongation of 20 per cent in 2 in., when the sample is taken in this manner.

Coupon tests in manganese bronze castings, though usually representative, do not necessarily represent the body of the casting, while tests actually taken from the casting will always give the true quality of the casting in question. It is much safer to use castings thus individually tested when they are to be subjected to high stresses.

METHOD OF SAMPLING AND ANALYSIS OF TIN, TERNE AND LEAD-COATED SHEETS.

BY J. A. AUPPERLE.

SUMMARY.

The object of this paper is to propose a method of sampling and analysis which will represent the average analysis of a lot of tin, terne and lead-coated sheets. Investigations have shown that the amount of sample taken for analysis varies greatly, and that there is practically no agreement in the method of sampling.

Investigations have also disclosed the fact that 90 per cent of the laboratories use the method substantially as described in this paper. In the determination of tin, it was found that some laboratories dissolve the plate directly in hydrochloric acid and titrate the tin. Others remove the coating from the base plate and determine the tin gravimetrically or by titration. It was also found that lead was determined electrolytically and as sulfate. In some cases this element was determined by difference.

We propose a method of sampling which shows a true average of the sheet. The methods of analysis are rapid and accurate and already in use in many laboratories.

The method of sampling herein described will give a larger and more truly average sample than most of the other methods we have investigated, and the methods of analysis are rapid and accurate. The removal of coating with hot sulfuric acid is a rapid test for weight of coating as well as a convenient means for analyzing the base metal.

METHOD OF SAMPLING AND ANALYSIS OF TIN, TERNE AND LEAD-COATED SHEETS.

By J. A. AUPPERLE.

In response to inquiries to the manufacturers and consumers of tin, terne and lead-coated sheets, we received information which showed there was no uniformity in the method of sampling of these materials, nor in the amount taken for analysis. As regards chemical analysis, we found that about 90 per cent of the replies received described the method which will be referred to hereafter as the "proposed method."

Tin, terne and lead-coated sheets invariably contain a ridge of heavy metal at one edge, due to the flow of metal after the sheets are coated. The manufacturer ordinarily bases the weight of coating upon the weight of a box of black sheets, and the weight of the same sheets after coating. The consumer is not interested in the amount of heavy metal on certain portions of the sheet known as "list edge;" that in which he is interested is the average weight of coating. Consequently, when an analysis is made disregarding the "list," a difference of several pounds per box may be found between the amount of metal the manufacturer puts on the sheet and the amount found by chemical analysis.

In order to standardize the methods of sampling, it is desirable to disregard the "list edge," and to take a sample which fairly represents the average of the sheet. From investigations which we have conducted, we believe that samples from near the four edges of the sheet accurately represent the average coating. We therefore propose the following method of sampling and methods of analysis:

PROPOSED METHOD OF SAMPLING AND ANALYSIS OF TIN, TERNE AND LEAD-COATED SHEETS.

METHOD OF SAMPLING.

Four 2 by 4-in. pieces are cut, one from each end and each side of the sheet, parallel with the sides and equidistant from

the ends, as shown in Fig. 1. One sheet from each grade or shipment is taken for analysis.

These samples, before weighing, should be thoroughly cleaned with chloroform, carbon tetrachloride or gasoline. Each piece is then cut in half, marking one half "A" and the other half "B." The four pieces comprising lot A are then accurately weighed together, cut into small pieces about $\frac{1}{8}$ in. square, thoroughly mixed, and used for the determination of tin and lead. The four pieces comprising lot B are reserved for the analysis of base metal and the direct determination of coating as a check on the analysis of lot A.

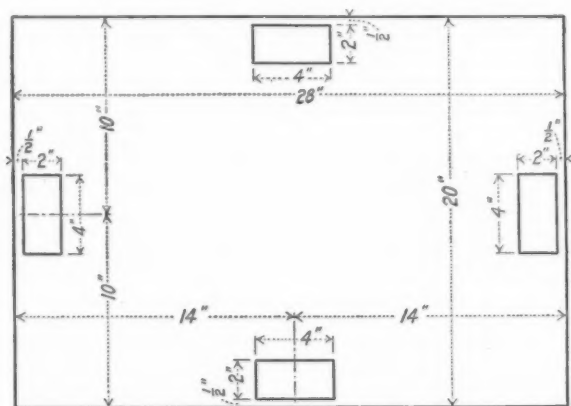


FIG. 1.

A templet should be provided, made preferably from steel $\frac{1}{8}$ in. thick and exactly 2 by 4 in. A scribe is used to accurately mark the sections to be cut. The templet is then used to subdivide the 2 by 4-in. specimens into two pieces, 2 by 2 in. The sections for analysis are then cut with tinner's shears.

DETERMINATION OF TIN.

Three 5-g. portions of the finely cut sample of lot A are placed into three 300-cc. Erlenmeyer flasks, each fitted with a one-hole rubber stopper containing a glass tube bent twice at right angles, one end of which projects through the rubber

stopper for a short distance, the other end being long enough to reach almost to the bottom of a beaker, placed on a level with the flask, containing about 300 cc. of dilute sodium-bicarbonate solution. Add 75 cc. of concentrated hydrochloric acid, connect the flask with the stopper containing the glass tube, and place the flask on a hot-plate. Heat gradually at first until most of the metal is in solution. The long end of the glass tube, in the meantime, is submerged in the beaker. The hydrochloric-acid solution is finally brought to boiling and when all the metal is dissolved the beaker containing dilute sodium-bicarbonate solution is replaced by one containing a saturated solution of the same. Remove the beaker and flask to a cool place. This will cause a small amount of the sodium bicarbonate to enter the flask and exclude the air. The solution is finally brought to a low temperature, preferably with ice water. This solution is then diluted to about 200 cc. with oxygen-free water which contains several cubic centimeters of starch solution, and titrated with N/20 iodine solution. We have found this strength of iodine solution to be the most satisfactory for this method.

The distilled water free from oxygen is obtained in any of three ways: (1) By passing carbon dioxide through the cold distilled water; (2) by boiling vigorously and cooling; or (3) by adding a few cubic centimeters of concentrated hydrochloric acid to the water and then about 2 g. of sodium bicarbonate, stirring vigorously. By running this determination in triplicate, the first titration serves as a control to indicate the number of cubic centimeters of iodine required, whence the two succeeding titrations may be made very rapidly and should check very closely.

Standardizing the Iodine Solution.—About 0.1 g. of pure tin and 4 g. of iron filings are dissolved in 75 cc. of concentrated hydrochloric acid, etc., as under the determination of tin. One cubic centimeter of N/20 iodine = 0.002975 g. of tin.

Calculation.—Weight of tin:

$$\frac{\text{Wt. of tin on 5 g.} \times \text{Wt. (g.) of 16 sq. in.}}{5} \times 8.6421 =$$

number of pounds per case of 112 sheets, 20 by 28 in.

DETERMINATION OF LEAD.

Dissolve 10 g. of the finely cut sample of lot A in 150 cc. of nitric acid (1 : 1). Heat until free from brown fumes and dilute to 1 liter and mix thoroughly. Take 100 cc. of this solution, add 10 cc. of concentrated nitric acid, electrolyze at a temperature of 50 to 60° C., using 1 to 2 amperes and 2.3 to 2.5 volts. The weight of PbO_2 is multiplied by 0.866.

If the base metal contains an appreciable amount of manganese the lead should be determined as sulfate.

Calculation.—Weight of lead:

$$\text{PbO}_2 \text{ found (g.)} \times 0.866 \times 10 = \text{Pb};$$

$$\frac{\text{Pb} \times \text{Wt. (g.) of 16 sq. in.}}{10} \times 8.6421 =$$

number of pounds per case of 112 sheets, 20 by 28 in.

DIRECT DETERMINATION OF THE WEIGHT OF COATING.

The remaining four pieces representing lot B are used for the analysis of the base metal and incidentally can be used for the direct determination of the weight of coating.

The four 2 by 2-in. pieces are carefully weighed together and each piece is wrapped with a stiff platinum or nickel wire in such a manner that it may be placed in the acid in a horizontal position. Heat 60 cc. of concentrated sulfuric acid contained in a 400-cc. Jena glass beaker to at least 250° C., immerse each piece separately in the hot acid for exactly 1 minute, and remove to a 600-cc. Jena beaker containing 50 cc. of distilled water. Immerse momentarily and rub the surface while washing with about 50 cc. more of distilled water, using a wash bottle for this purpose. The four samples are thoroughly dried, reweighed, and used for the analysis of base metal.¹

The loss in weight represents the coating and some iron. The sulfuric acid contained in the 400 cc. beaker is cooled and combined with the washings in the 600 cc. beaker. Two hundred cubic centimeters of concentrated hydrochloric acid are added and the solution boiled for a few minutes. The solution is cooled, poured into a graduated 500 cc. flask and filled to the mark with distilled water.

¹ The methods of analysis of the base metal are outside the scope of this paper and will not be given.

Determination of Iron.—Place 100 cc. of this solution in a 300-cc. Erlenmeyer flask, add 1 cc. of a saturated solution of potassium permanganate to oxidize the iron and tin, heat to boiling and reduce with a few drops of stannous chloride. Cool, pour into a liter beaker containing 400 cc. of distilled water, add 25 cc. of mercuric chloride, followed by 10 cc. of phosphoric acid and manganese-sulfate solution, and titrate with N/10 potassium permanganate.

Calculation.—

Four pieces 2 by 2 in. weigh.....	28.5686 g.
Same after stripped in acid.....	24.1620 "
Loss, coating plus iron.....	4.4066 "
Iron as found by titration.....	0.4887 "
Weight of coating.....	3.9179 "

$3.9179 \times 8.6421 =$ number of pounds per case of 112 sheets, 20 by 28 in.

$\frac{\text{Tin in 100 cc.} \times 5 \times 100}{\text{Weight of Coating}} =$ percentage of tin.

Percentage of lead is obtained by difference.

In the analysis of tin plate, the weight of coating is expressed in pounds per box, which is a half case, or 112 sheets 14 by 20 in.; hence to obtain the weight of coating per box on tin plate, the number of pounds as obtained above is divided by two.

Check Determination of Tin.—The remainder of the solution which has been used for the determination of iron can be used for the determination of tin as follows: Place three portions of 100 cc. each in three 300-cc. Erlenmeyer flasks. If any of the lead sulfate should or should not be removed in any of these portions, the accuracy of the tin determination is not affected. Add 1 g. of powdered antimony, connect with rubber stopper and glass tube described in the method of determination of tin in the sample of lot A, place on a hot-plate, using dilute sodium-bicarbonate solution as a trap, and boil until the solution becomes decolorized. Replace the dilute sodium-bicarbonate solution with a saturated solution of the same, remove from the hot-plate, cool, dilute and complete the determination as described under the first method.

CONCLUSIONS.

We claim for this method that the sample shows a true average of the coating on the plate, since we have checked the coating very closely by this method and by sampling from the center of the sheet, even with such large samples as 10 by 10 in. When 5 g. of the sample are taken for the determination of tin, an area of about 2.5 sq. in. is represented in the case of 40-lb. IC plate, and of about 3 sq. in. in the case of 25-lb. plate; while, of course, it is double this in the determination of lead. Furthermore, the amount of sample taken here for analysis is a representative quantity from 16 sq. in. and not merely from one particular section of 2.5 or 3 sq. in.; and is as large as many laboratories are using and larger than most are using.

In addition, we believe that this method is more truly average than any method we have investigated, and moreover, the sheet is not destroyed so far as usefulness is concerned, but may be sheared down to a smaller size. While it is not necessary to determine the weight of coating directly by the sulfuric acid method, in addition to the determination of the lead and tin (on lot A), it will, however, serve as a check, and should agree very closely with it. Furthermore, this is an excellent method for stripping the coating preliminary to the analysis of the base metal.

By running the determination of tin in triplicate, as described, the method is very rapid and accurate, whereas the method as now used by many laboratories in which the plate is dissolved in an atmosphere of carbon dioxide in a graduated flask, cooled, diluted to volume and titrated in aliquots, involves many details and is not so rapid. In this method also, no antimony is needed for the reduction of tin, since the iron in the base metal accomplishes this; moreover, in the presence of the quantity of tin here involved the antimony would have a tendency to deposit back on the plate, retarding the solution of the tin and thus giving low results.

With the use of a rotating anode the proposed method is very rapid and the entire determination can be finished in a reasonable length of time.

DISCUSSION.

MR. S. S. VOORHEES.—I have read the paper of Mr. Aupperle Mr. Voorhees. with much interest. It came too late for us to make many determinations, but on such tests as we have made, we found a very reasonable agreement. The difference in the method of sampling between our method and that of Mr. Aupperle is that the sample is taken from the center of the sheet. We take a sheet of 36 sq. in., cut it up into small squares, and determine lead from a 10-g. sample in the manner described by Mr. Aupperle, except that we dry the peroxide of lead at a somewhat higher temperature. In spite of all precautions, we found that the lead determined as peroxide is usually higher than when it is converted to the sulfate.

For determination of tin, our method is, I think, somewhat shorter. We dissolve 10 g. of the sample in hydrochloric acid in the presence of marble so as to insure an atmosphere of CO_2 , and make up to a fixed volume; but in place of titrating an aliquot part of that solution with iodine, as was originally described by Mr. Job before the Society of Chemical Industry, we reverse the procedure and draw up the solution of stannous chloride into a burette, and run that into a fixed quantity of iodine. This requires only one determination, and if proper precautions are taken, there is a minimum opportunity for oxidation. By comparison of results obtained by our methods and the methods recommended by Mr. Aupperle, we get somewhat higher results with our methods, but on checking our methods against a solution containing a known amount of tin and lead, we got consistent results. In the stripping of the plates to obtain a sample for analysis of the base, we strip in a 10-per-cent solution of caustic soda, using the plate as an anode with a current of three to five ampères by which the tin goes into solution, and the lead is deposited in part as spongy metal. From a comparison of a large number of results of stripping by this means and by our method of analysis, the results are in most cases somewhat higher by actual analysis than by stripping, which is

Mr. Voorhees. probably due to an alloy of the coated metal with the base, which is not removed in the alkali bath, but which is caught by analysis. I hope to make further comparative tests of both the method of sampling and analysis and report later by letter. It seems that it will be necessary to arbitrarily decide on the method of sampling, as there is a marked variation in the quantity of coatings in different parts of the sheet. Therefore it will only be by such means that uniformity of results can be obtained.

Mr. Aupperle. **MR. J. A. AUPPERLE.**—I concur with what Mr. Voorhees has said in reference to the titration of tin, and I believe that his modification is very good, but we have been accustomed to running it as described and for that reason have recommended it, especially as we have found quite a number of other laboratories using it in pretty nearly the same manner. I believe what Mr. Voorhees says about making an arbitrary method of sampling is also very good; that is, if we establish a certain location of the sheet from which to cut the samples, there will be more probability of accurate chemical analysis.

Mr. Campbell. **MR. J. H. CAMPBELL** (*by letter*).—Mr. Aupperle's proposed methods for sampling and analysis of tin, terne and lead-coated sheets should give an accurate and rapid determination of the weight of the coating, and the weight of the constituent metals. The electrolytic method for determining lead, while rapid and accurate, requires a considerable outlay for anodes and cathodes of platinum, and where a large number of determinations are to be rapidly made in one day, the present high price of platinum would make this method rather expensive. In our laboratory we use the following method for lead:

A 4 by 4-in. sheet is cleaned with gasoline and immersed in 80 cc. of hot 1-to-3 nitric acid, and allowed to remain until the lead is dissolved. The metal is removed as soon as this appears to be accomplished, so that the amount of iron dissolved may be a minimum. Clean the sheet by washing with distilled water and rubbing. Evaporate the main solution and washings to about 50 cc. and filter off any tin that may be precipitated. To the filtrate add 5 to 10 cc. of concentrated sulfuric acid and fume; dilute, add 10 per cent of the volume of the filtrate of alcohol and allow to stand several hours; filter off the lead sulfate, dry and weigh. This gives the weight of lead on 16 sq. in.

of metal, from which the amount may be calculated in pounds **Mr. Campbell.** per case of 112 sheets, for any size of sheet.

The tin we determine on a sheet of the same size, using the method given by Mr. Aupperle.

The objections to selecting a 4 by 4-in. piece from the center of the sheet are well taken, in so far as the sheet would be destroyed if this procedure were followed, while by using the method indicated by the author—getting far enough into the sheet to avoid the ridge of heavy coating due to the draining of the plate—this objection would be overcome.

ERRORS IN THE METHODS OF DETERMINING THE TIME OF SETTING OF CEMENT.

By G. M. WILLIAMS.

SUMMARY

The time of setting of cement is affected by a number of factors, which acting together or separately account for the variable results obtained on the same material in different laboratories.

The variation in results obtained in this investigation are only to be considered as characteristic of the range which may be obtained in practice.

The following factors may cause errors of considerable magnitude:

1. Variation in the amount of work done on the material may cause a difference of more than two hours in the time of initial setting and cause a normal cement to appear quick setting;
2. Variation in atmospheric moisture or humidity of storage during the setting period may cause the initial time of setting to vary as much as two hours;
3. Variation in atmospheric heat or temperature of storage during the setting period may vary the time of setting as much as 1 or 2 hours.

The determination is also affected, to a less extent, by factors peculiar to the Vicat and Gillmore methods.

Conclusions.—The important factors to be considered are the working of the cement paste, and the temperature and humidity of the damp closet. The other variables, such as formation of the test specimen and manipulation of apparatus, are of smaller importance compared with those of mixing and curing, but these errors may combine to increase those caused by the latter.

The tests as made at present can be relied upon only to identify normal or slow-setting cements, and may cause a normal cement to appear quick setting.

The results indicate that neither the Vicat nor Gillmore methods for measuring the rate of hardening, even when all external factors are controlled, will give results consistent enough to justify the reporting of such results within the limits of a few minutes; nor can either method be considered as sufficiently accurate for use as a standard.

ERRORS IN THE METHODS OF DETERMINING THE TIME OF SETTING OF CEMENT.

BY G. M. WILLIAMS.

The determination of the rate of hardening, or setting of Portland cement, after gaging with water is affected by a number of external factors, which, acting separately or together, account for variable results obtained on the same material in different laboratories.

METHODS OF MEASURING TIME OF SET.

The means of measuring the time of setting or rate of hardening have been arbitrarily established in practice, and are defined as follows:

Vicat Method.—A paste of normal consistency is mixed and molded into a test specimen 4 cm. thick and 7 cm. in diameter at the base, and placed under a rod weighing 300 g. bearing a needle 1 mm. in diameter. The initial set is said to have occurred when the needle ceases to pass a point 5 mm. above the bottom of the test specimen, and the final set, when the needle does not sink visibly into the paste.

Gillmore Method.—The cement is said to have acquired its initial set when a pat 3 in. in diameter by $\frac{1}{2}$ in. in thickness will bear, without appreciable indentation, a needle $\frac{1}{16}$ in. in diameter loaded to weigh $\frac{1}{4}$ lb. The final set has been acquired when the pat will bear without appreciable indentation, a needle $\frac{1}{4}$ in. in diameter loaded to weigh 1 lb.

Although the specifications require that "initial" and "final" set be attained within rather wide limits of time, namely, 30 minutes and 10 hours, it is common practice in reporting the results of this determination to state the values obtained within narrow limits of 1, 5 or 10 minutes. Since it is difficult to reproduce the results from day to day on the same sample of material in the same laboratory, it is not to be expected that the results will agree with those obtained in another

laboratory where the test may be made under widely different conditions of temperature, humidity and methods of manipulation.

Most normal cements are usually well within the limits specified for time of setting when tested with either the Vicat or Gillmore needles, and it is only at intervals that cements are found to be near the specification limits. Therefore, for normal cements the variations obtained are of small importance, but when a very quick or very slow-setting cement is found, it is necessary that the method used be accurate or that the limits of accuracy of each of the factors entering into the determination be known.

EFFECTS OF VARIABLES.

To determine the effect of the different external variables upon the rate of hardening of cement, it is necessary that each factor be considered separately.

The following are the most important:

1. Effect of slight errors in weighing the cement and measuring the mixing water;
2. Effect of variation in the amount of work done in mixing the cement;
3. Effect of atmospheric moisture or relative humidity of storage during the setting period;
4. Effect of heat, or atmospheric temperature of storage, during the setting period.

The above factors enter into the results irrespective of the method used for measuring the rate of hardening. The determination is also affected by factors peculiar to the Vicat and Gillmore methods, as follows:

With the Vicat method:

5. Effect of additional work in the formation of the test specimen;
6. Effect of dirty and bent needles;
7. Effect of releasing the needle above the surface of the test specimen;
8. Effect of lack of homogeneity of the mass and variation in observing the indentation of the needle.

With the Gillmore method:

9. Effect of additional work in the formation of the test specimen;

10. Effect of not applying the needles squarely upon the surface of the test specimen.

11. Effect of variations in observing the end point of indentation of the needles.

To obtain some idea of the variation of setting time due to the above factors, test specimens were prepared and allowed to harden with all conditions constant, in so far as possible, except the one whose effect was to be noted. In all cases where comparisons are made between different results the same cement

TABLE I.—EFFECT OF SLIGHT ERRORS IN WEIGHING THE CEMENT AND MEASURING THE MIXING WATER.

Cement No.	Percentage of Mixing Water.	Vicat Method.		Gillmore Method.	
		Time of Initial Set. ^b	Time of Final Set.	Time of Initial Set.	Time of Final Set.
D-2	21.5	1:48—1:51	4:58	2:07	4:48
	23.5 ^a	2:20—2:33	5:34	2:50	5:45
	25.5	3:35—3:42	7:19	3:38	6:59

^a Normal consistency.

^b Initial set first observed at time of first penetration to a point 5 mm. above plate, which is given by the first figure. Initial set was completed when penetration to a point 5 mm. could not be secured, given by last figure. For example, initial set was first observed at 1:48; completed at 1:51; elapsed time 3 minutes.

was used in each specimen, and all factors not otherwise mentioned were uniform.

The variation in the results obtained in this particular cement are not to be considered as the maximum which may occur in practice, but are only to be considered as characteristic of the range of values which may be obtained under variable conditions similar to those noted.

1. *Effect of Slight Errors in Weighing the Cement and Measuring the Mixing Water.*—Since an excess of mixing water is equivalent to a reduction in the amount of cement, only one of these factors need be considered. As indicated in Table I, the rate of hardening is decreased by an increase in the amount of water, or, the setting is retarded. By increasing the percentage

of water from 21.5 to 25.5 per cent, the initial set is retarded $1\frac{1}{2}$ to 2 hours, and the final set 2 hours, approximately. The above percentages of water were probably the extremes that could have been used for this particular cement, which required 23.5 per cent for normal consistency by the ball method.

2. *Effect of Variation in the Amount of Work Done in Mixing the Cement.*—The results reported in Table II show conclusively that it is necessary to define more definitely the expression "vigorously," since the quantity of energy expended, and expressed by "vigorously" and "less vigorously," has been understood as

TABLE II.—EFFECT OF VARIATION IN THE AMOUNT OF WORK DONE IN MIXING THE CEMENT.

Cement No.	How Worked.	Vicat Method.		Gillmore Method.	
		Time of Initial Set.	Time of Final Set.	Time of Initial Set.	Time of Final Set.
D-3	Vigorously, $1\frac{1}{2}$ min.....	1 : 43—2 : 01	2 : 37
	Less vigorously, $1\frac{1}{2}$ min...	0 : 26—0 : 41	1 : 13
2338	Vigorously.....	2 : 24	5 : 14	2 : 29	4 : 44
	Less vigorously.....	0 : 10	3 : 50	1 : 10	4 : 40
2349	Vigorously.....	3 : 03	6 : 27	3 : 08	6 : 27
	Less vigorously.....	1 : 23	6 : 18	1 : 48	5 : 23
2360	Vigorously.....	1 : 37	6 : 18	2 : 12	6 : 02
	Less vigorously.....	0 : 35	6 : 00	0 : 50	5 : 10
2352	Vigorously.....	2 : 15	4 : 45	2 : 20	4 : 50
	Less vigorously.....	0 : 35	4 : 10	0 : 50	6 : 00

NOTE.—Normal consistency used for each cement.

being in accord with the prescribed method. The initial time of setting under these conditions varied as much as 1 or 2 hours. The results obtained by vigorously working correctly indicate that the cement was normal in regard to the time of setting, while three of the four less vigorously worked Vicat specimens attained the initial set in less than 45 minutes, and two in less than 30 minutes. The Gillmore specimens did not set quite as rapidly, probably because of the fact that the pat received a little additional working while being formed. The results in Table II are shown graphically in Fig. 1.

The effect upon the final set is less marked.

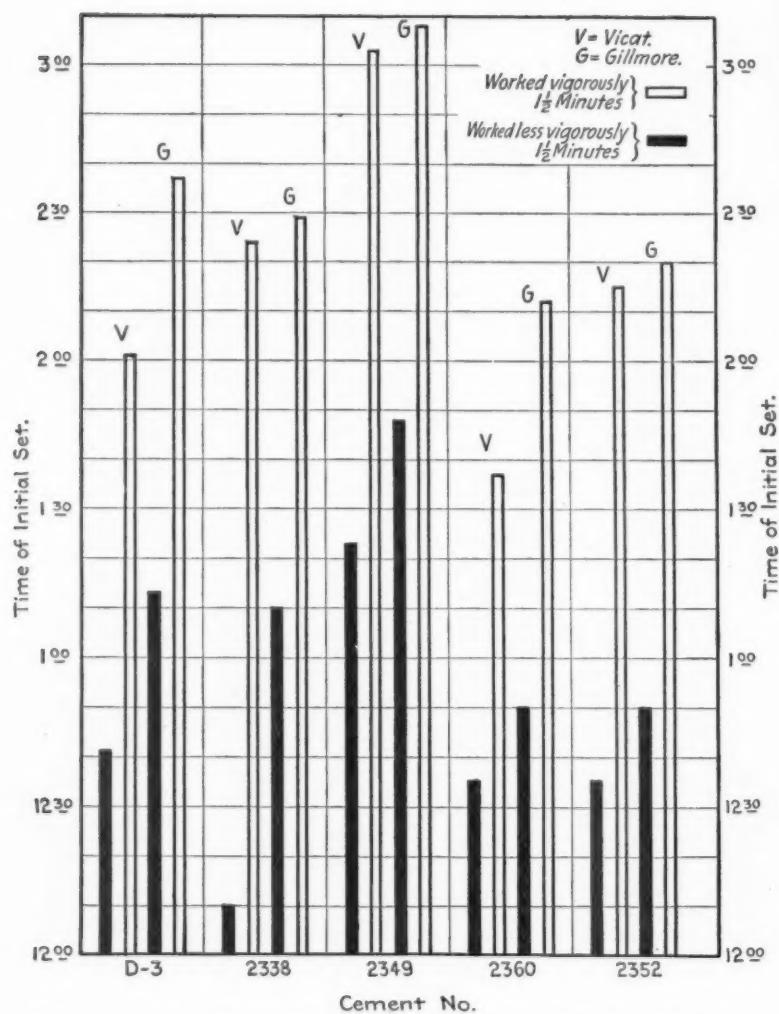


FIG. 1.—Effect of Variation in the Amount of Work Done in Mixing the Cement; Plot of Table II.

Table III shows the effect of continuing the vigorous working for various periods up to 4 minutes. The results indicate that excessive work on a normal cement tends to slightly shorten the time of setting, especially the final set.

A comparison of the results shown in Tables II and III indicate that a point is reached in the mixing of the mass where additional mixing has little effect on the time of setting, and tends to reduce the time of final set only slightly. Before this critical point is reached the reduction in setting time is marked. It is well known that a mass becomes more plastic after the working

TABLE III.—EFFECT OF VARIATION IN THE AMOUNT OF VIGOROUS WORKING OF THE CEMENT.

Cement No.	Time Worked, min.	Vicat Method.		Gillmore Method.		Temperature, deg. Cent.	Humidity (approx.), per cent.
		Time of Initial Set.	Time of Final Set.	Time of Initial Set.	Time of Final Set.		
D-2	1½	1:41—1:47	3:18	1:39	3:27	41—42	32
	4	1:32—1:36	3:16	1:40	3:05	
	1½	1:41—1:48	3:56	1:44	3:27	41—42	32
	4	1:36—1:39	3:23	1:46	3:02	
	1½	2:26	4:55	3:30	5:30	29—30	68—72
	4	2:02	4:39	3:13	4:50	
	1½	2:04—2:12	5:12	3:38	5:56	29—30	85
	4	1:45—1:53	4:20	3:32	5:00	
	1½	1:40—1:45	3:00	2:26	4:00	39—40	70—75
	4	1:38—1:41	2:52	2:28	3:25	
	1½	1:27—1:31	2:58	1:59	3:31	39—40	18—28
	4	1:27—1:30	2:54	1:58	3:10	

NOTE.—Normal consistency used in all cases.

and kneading has been continued for some time. This does not occur as a gradual change, and after reaching this plastic condition, the mass does not become appreciably more plastic as the working is continued. This critical point marks the change from a condition which would be recorded as quick setting to one which is recorded as normal. The length of time required to bring about this change depends upon the amount of energy used in the mixing. To make allowance for this factor, it would be well to specify more definitely how the kneading action should be carried out, as well as to make the time through which it should be continued simulate actual service.

3, 4. *Effect of Atmospheric Moisture or Relative Humidity of Storage, and the Effect of Heat, or Atmospheric Temperature of Storage, during the Setting Period.*—The results tabulated in Tables IV, V and VI point out that probably the largest factor affecting the time of setting is temperature, closely followed by humidity, assuming that sufficient work has been put upon the cement.

TABLE IV.—EFFECT OF ATMOSPHERIC MOISTURE OR RELATIVE HUMIDITY OF STORAGE, AND THE EFFECT OF HEAT OR ATMOSPHERIC TEMPERATURE OF STORAGE, DURING THE SETTING PERIOD.

GILLMORE METHOD.

Temperature, 29 to 30° C.

Cement No.	Relative Humidity, per cent.	Worked 1½ min.		Worked 4 min.	
		Time of Initial Set.	Time of Final Set.	Time of Initial Set.	Time of Final Set.
D-2	25—30	2 : 25	5 : 05	2 : 17	4 : 30
	38—42	2 : 35	5 : 28	2 : 31	4 : 36
	38—42	2 : 35	5 : 29
	38—45	2 : 38	5 : 28
	38—45	2 : 38	5 : 26
	50—55	2 : 48	5 : 18	2 : 32	4 : 41
	60—70	2 : 52	5 : 30
	60—70	2 : 54	5 : 32
	68—72	3 : 30	5 : 50	3 : 13	4 : 50
	85	3 : 38	5 : 56	3 : 22	5 : 00

Temperature, 39 to 40° C.

D-2	18—28	1 : 50	3 : 31	1 : 58	3 : 10
	20—30	1 : 50	3 : 45	1 : 57	3 : 21
	20—30	1 : 50	3 : 29
	20—30	2 : 01	3 : 52
	32	1 : 39	3 : 27	1 : 40	3 : 05
	32	1 : 44	3 : 27	1 : 46	3 : 02
	33	1 : 54	3 : 20	1 : 43	3 : 00
	33	1 : 55	3 : 47	1 : 56	3 : 18
	70—75	2 : 26	4 : 00	2 : 28	3 : 25
	70—80	2 : 36	3 : 46

A high temperature causes a more rapid evaporation of moisture from the test specimen and aids in hastening the hardening of the mass. A high relative humidity tends to prevent rapid drying, and consequently retards the setting action.

To obtain the results shown in Tables IV, V and VI, the test specimens were placed in a closed vessel, and the temperature and relative humidity held within the limits indicated throughout the test.

As would be expected, the humidity has less influence on the Vicat form of test specimen than on the Gillmore, due to its larger mass and the smaller area of exposed surface.

A comparison of the results obtained in the different temperatures shows that the two forms of test specimens are equally affected by temperature variations.

TABLE V.—EFFECT OF ATMOSPHERIC MOISTURE OR RELATIVE HUMIDITY OF STORAGE, AND THE EFFECT OF HEAT OR ATMOSPHERIC TEMPERATURE OF STORAGE, DURING THE SETTING PERIOD.

VICAT METHOD (STANDARD TEST SPECIMENS).

Temperature, 29 to 30° C.

Cement No.	Relative Humidity, per cent.	Worked 1½ min.		Worked 4 min.	
		Time of Initial Set.	Time of Final Set.	Time of Initial Set.	Time of Final Set.
D-2	25-30-2:20	5:07-2:18	4:45
	32-42	2:28-2:38	5:47
	32-42	2:29-2:44	5:42
	28-45	2:31-2:37	5:39
	28-45	2:28-2:36	5:29
	38-42	2:28-2:38	5:47
	38-42	2:29-2:44	5:42
	38-50	2:16-2:23	5:49
	38-50	2:29-2:35	5:58
	38-50	2:07-2:20	5:35
	42-52-2:38
	42-52	2:38-2:51
	42-52	2:23-2:32	5:54
	42-52	2:31-2:41	5:46
	50-55	2:30-2:35	5:20-2:20	4:51
	68-72	2:26	4:55	2:02	4:39
	85	2:04-2:12	5:12	1:45-1:53	4:20

Temperature, 39 to 40° C.

D-2	18-28	1:27-1:31	2:58	1:27-1:30	2:54
	20-30	1:42-1:49	4:05	1:30-1:34	3:13
	20-30	1:30-1:36	3:33
	20-30	1:51-1:56	3:57
	32	1:41-1:47	3:48	1:32-1:36	3:16
	32	1:41-1:48	3:56	1:36-1:39	3:23
	33	1:53-1:56	3:33	1:49-1:55	3:05
	33	1:52-1:55	3:34-1:51	3:04
	70-75	1:40-1:45	3:00	1:38-1:41	2:52
	70-80	1:40-1:57	3:24

VICAT VARIABLES.

5. *Effect of Additional Work in the Formation of the Test Specimen.*—After cutting off the excess material above the rubber ring as required by the specifications, enough was replaced and troweled in to completely fill it, leaving a very smooth surface. The effect of this extra work was to retard the set

TABLE VI.—EFFECT OF ATMOSPHERIC MOISTURE OR RELATIVE HUMIDITY OF STORAGE, AND THE EFFECT OF HEAT OR ATMOSPHERIC TEMPERATURE OF STORAGE, DURING THE SETTING PERIOD.

GILLMORE METHOD.

Temperature, 22 to 24° C.

Cement No.	Relative Humidity, per cent.	Time of Initial Set.	Time of Final Set.
U-2	20	2 : 29	6 : 32
	50	2 : 49	7 : 01
	90—100	4 : 24	7 : 01
	24	2 : 29	7 : 29
	78—82	3 : 26	6 : 39
	90—100	4 : 23	7 : 45
	24	2 : 21	6 : 43
	80	3 : 17	6 : 45
	90—100	4 : 13	7 : 01

Temperature, 29 to 30° C.

D-2	60—70	2 : 52	5 : 30
	60—70	2 : 54	5 : 32
	90—100	3 : 44	5 : 41
	90—100	3 : 46	5 : 46
	38—42	2 : 35	5 : 43
	38—42	2 : 39	5 : 44
	90—100	3 : 52	6 : 01
	90—100	3 : 55	6 : 03

TABLE VII.—EFFECT OF ADDITIONAL WORK IN THE FORMATION OF THE TEST SPECIMEN.

VICAT TEST SPECIMEN.

Cement No.	Method of Forming Test Specimen.	Time of Initial Set.	Time of Final Set.
D-2	A	2 : 28—2 : 38	5 : 47
	B	2 : 55—3 : 03	5 : 53
	A	2 : 29—2 : 44	5 : 42
	B	2 : 44—2 : 53	5 : 48
	A	2 : 23—2 : 32	5 : 54
	B	2 : 33—2 : 41	5 : 53
	A	2 : 31—2 : 41	5 : 46
	B	2 : 46—2 : 59	5 : 58
	A	2 : 16—2 : 23	5 : 49
	B	2 : 37—2 : 42	5 : 51

NOTE.—(A) Test specimen formed according to specifications.

(B) Test specimen formed according to specifications, with enough material replaced to completely fill mold, and then troweled smooth.

slightly but gave apparently the same uniformity in results of the initial set, and aided in the determination of the final set, due to the surface being smoother.

The results obtained on the specimens worked in this manner are compared in Table VII with those obtained on the standard Vicat test specimens.

The Vicat test specimen, if made strictly according to the specification, often has a surface which is not plane, and slight errors such as the above are introduced in determining the final time of setting, unless the test specimen is so adjusted that the surface is at right angles to the movement of the needle.

6. *Effect of Dirty and Bent Needles.*—The effect of a dirty and bent needle is shown in Table VIII. The first result shown

TABLE VIII.—EFFECT OF DIRTY AND BENT VICAT NEEDLES.

VICAT TEST SPECIMEN.

Cement No.	Condition of Needle.	Time of Initial Set.
D-2	A	2 : 33—2 : 45
	B	Complete at 2 : 59
	C	2 : 25—2 : 38
	B	Complete at 2 : 40

NOTE.—(A) Needle dirty, and bent so as to leave a $\frac{1}{8}$ -in. elliptical hole in the test specimen.

(B) Needle straight and clean.

(C) Same condition as (A) except less bend in needle; $\frac{1}{8}$ -in. elliptical hole in test specimen.

is an extreme case and probably would rarely be found in practice.

The second result, obtained under conditions which may be found in practice, indicate that this factor is of small importance as compared with some of the other variables.

7. *Effect of Releasing the Needle above the Surface of the Test Specimen.*—Fig. 2 illustrates the effect of releasing the needle 1 mm. above the surface of the test specimen. The scale readings of the instrument are plotted as the ordinates and the elapsed time as the abscissas. In every case each set of these readings was taken about $\frac{1}{4}$ in. apart.

8. *Effect of Lack of Homogeneity of the Mass and Variation in Observing the Indentation of the Needle.*—That the rate of the hardening of cement is not uniform throughout the mass of the test specimens is shown by Figs. 3, 4, 5 and 6. These

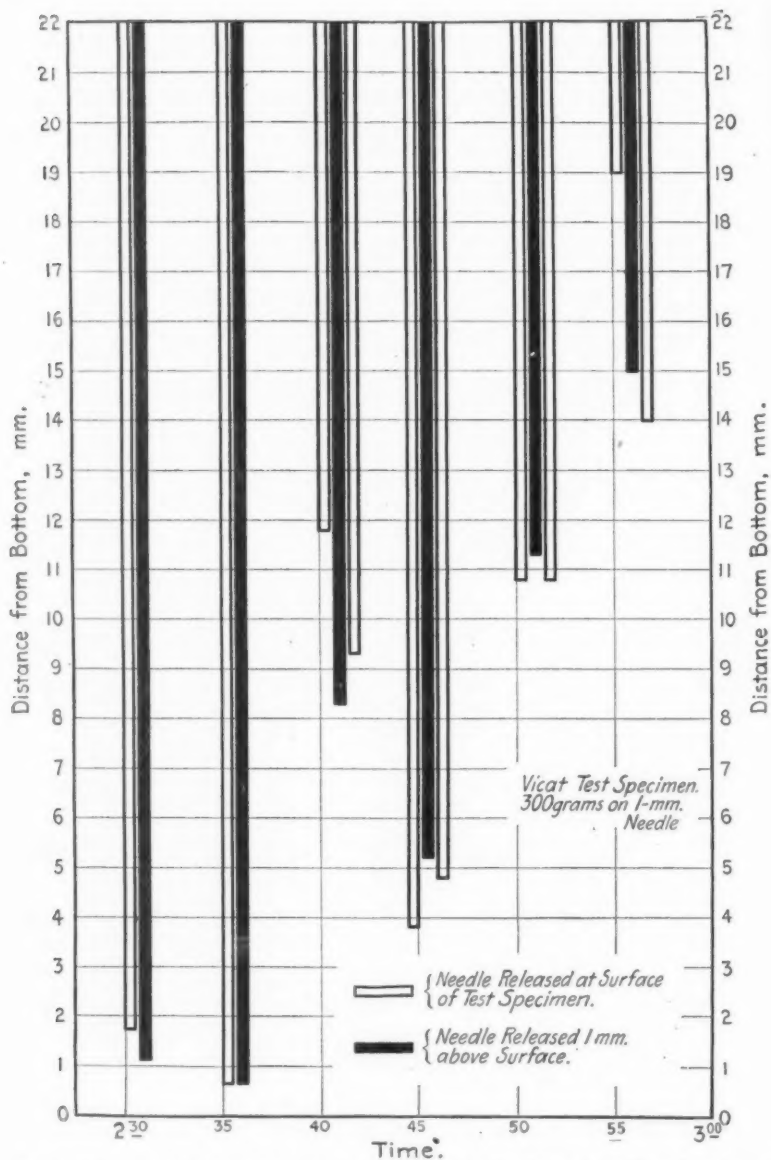


FIG. 2.—Effect of Releasing the Needle above the Surface of the Test Specimen.

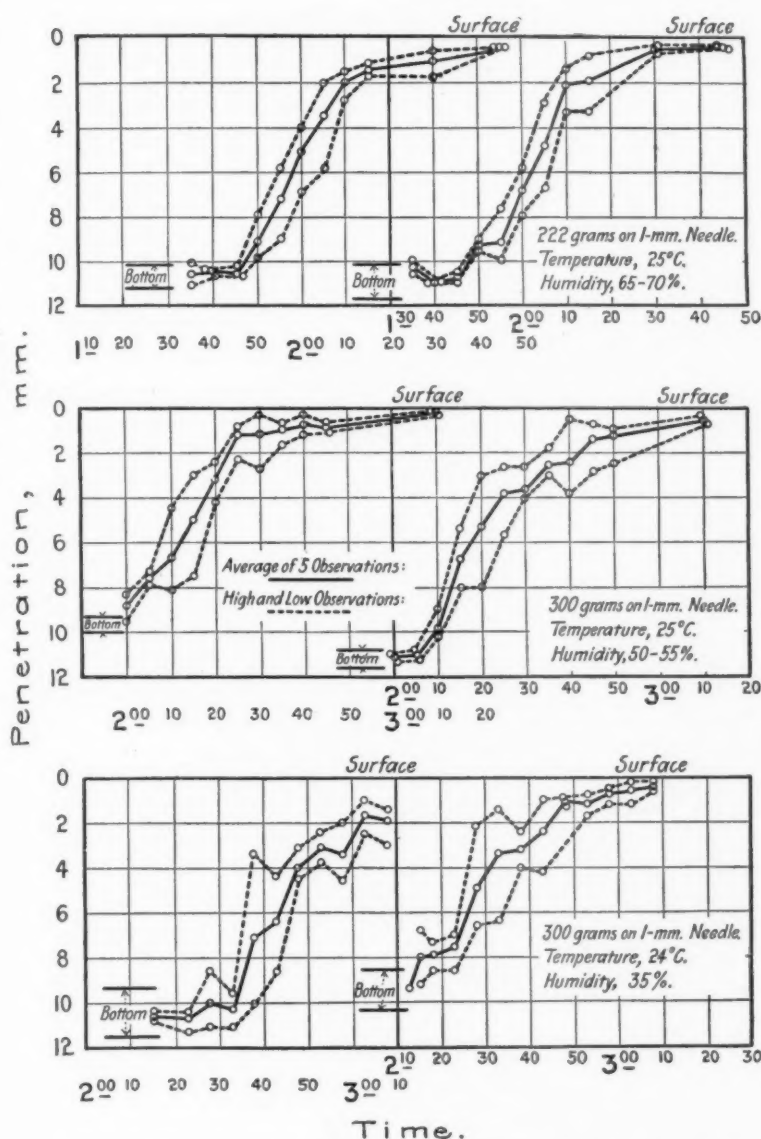


FIG. 3.—Effect of Lack of Homogeneity of the Mass and Variation in Observing Indentation of the Needle.

curves were obtained by noting the penetration of a weighted needle in test specimens of the Vicat and Gillmore types during

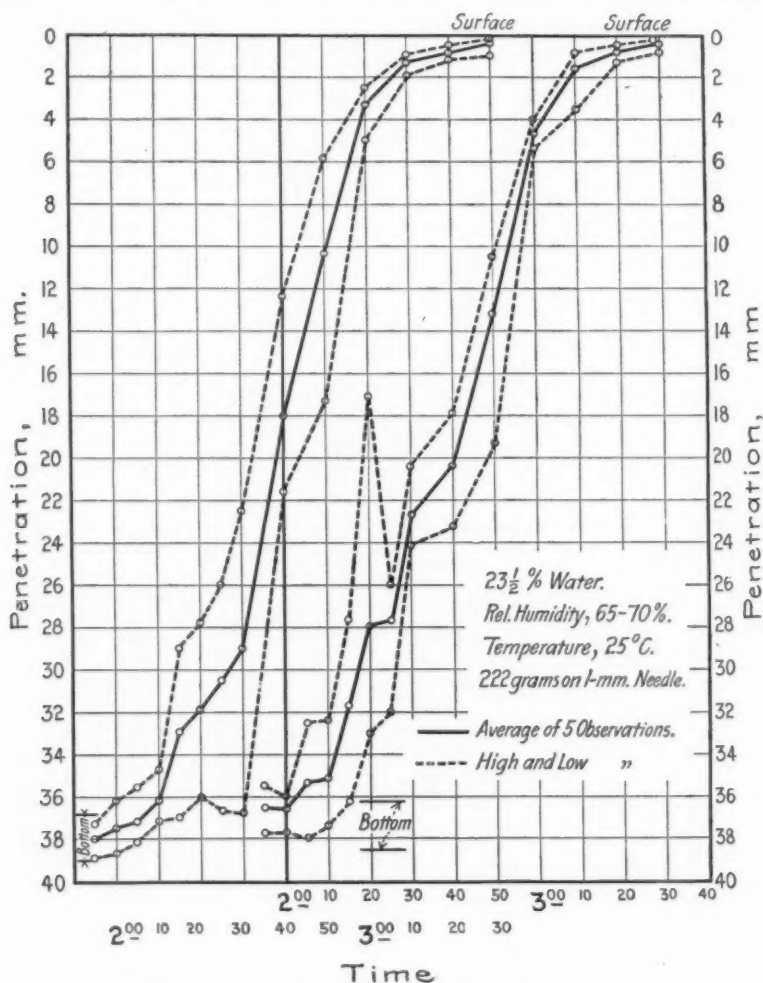


FIG. 4.—Effect of Lack of Homogeneity of the Mass and Variation in Observing Indentation of the Needle.

the period of hardening. For convenience in measuring the penetrations a Vicat scale and plunger were used. In the curves the penetrations in millimeters are plotted as the ordinates

and the elapsed time as the abscissas. Maximum and minimum penetrations, and averages are plotted. Five separate readings, distributed over the area of the test specimens, were used in ob-

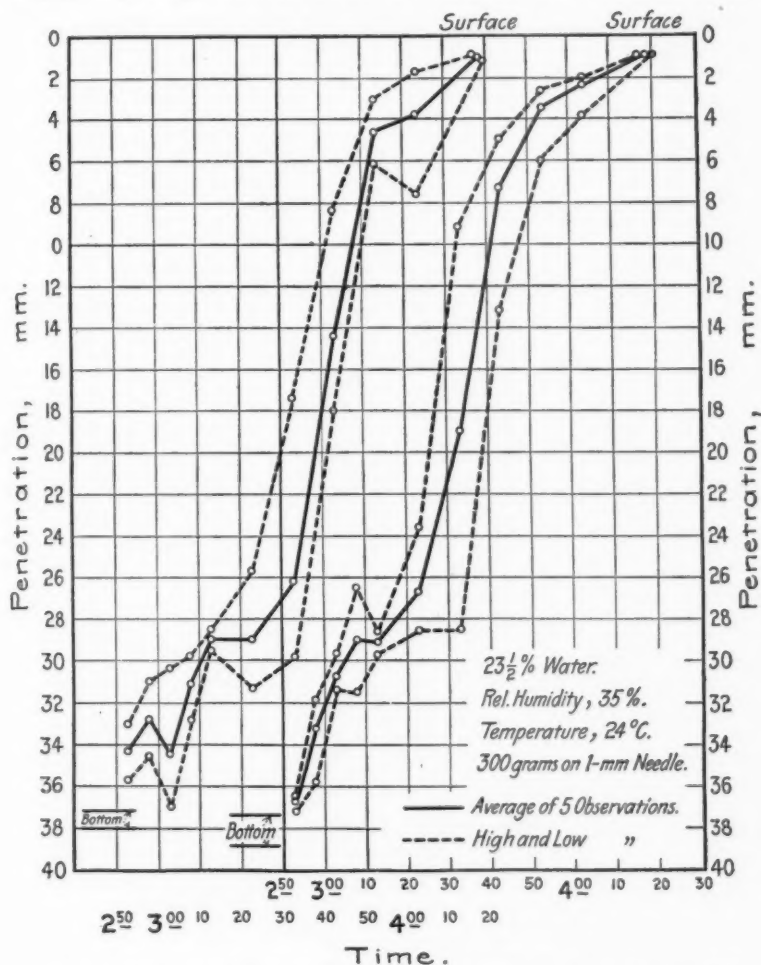


FIG. 5.—Effect of Lack of Homogeneity of the Mass and Variation in Observing Indentation of the Needle.

taining the averages in each case. Since actual penetrations were plotted and the test specimens varied slightly in thickness, the distance from surface of pat to the glass plate varied be-

tween the limits indicated by the two points marked "bottom" on each curve.

Fig. 7 was obtained in the manner of those preceding, except

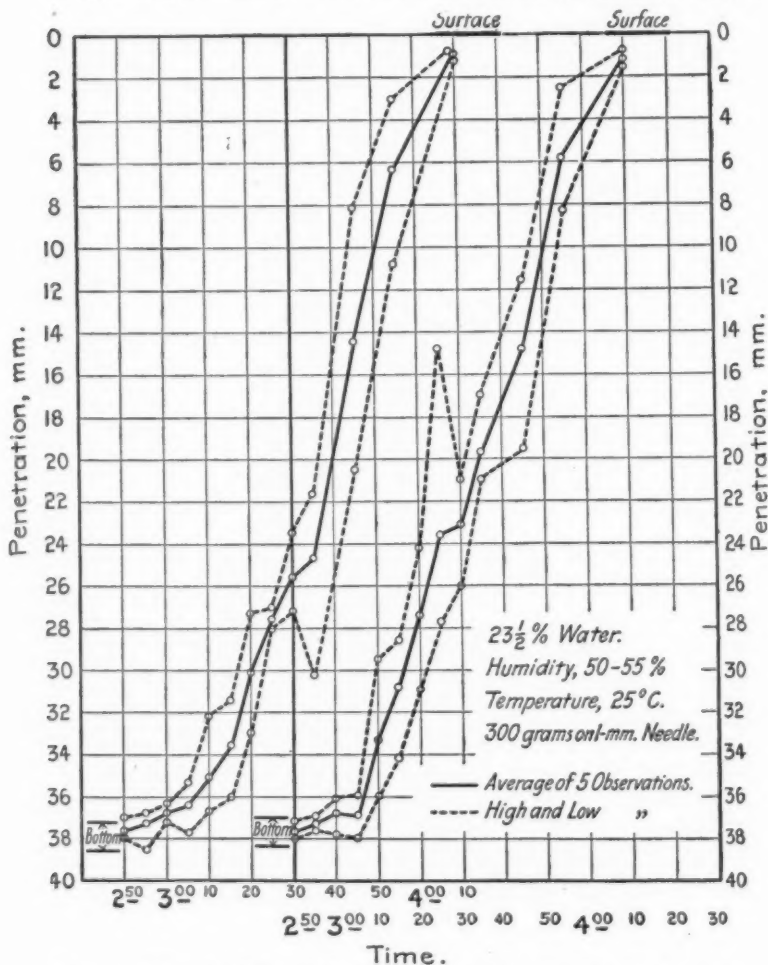


FIG. 6.—Effect of Lack of Homogeneity of the Mass and Variation in Observing Indentation of the Needle.

that the test specimen was divided into circular zones and one reading was obtained in each zone for each interval of time. The results indicate that the hardening is slightly more rapid in the center zone than near the outer edges of the specimen.

These curves show clearly the non-uniformity of the hardening of the mass, and account for the saw-tooth curve effects sometimes obtained by machines used for automatically

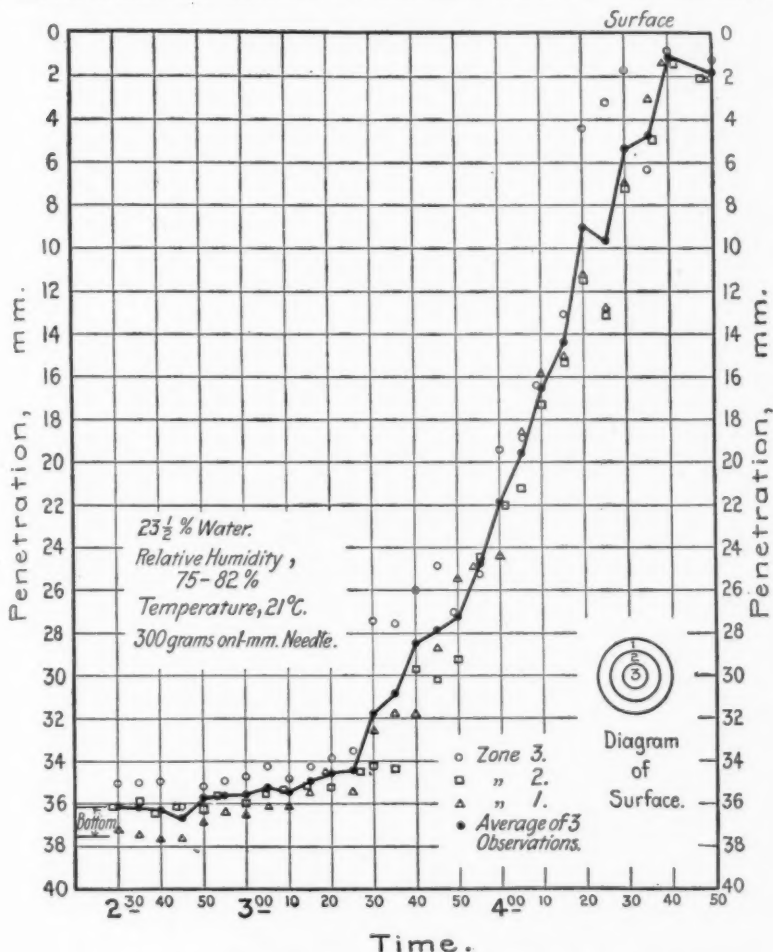


FIG. 7.—Effect of Lack of Homogeneity of the Mass and Variation in Observing Indentation of the Needle.

recording the time of setting. The variation in penetration varied from 0.0 to 17.0 mm., in one case. The time of initial set for these test specimens, if based on one observation, might vary

over periods of 20 minutes in the hands of different observers, depending upon the particular point selected for trial. The

TABLE IX.—EFFECT OF ADDITIONAL WORK IN THE FORMATION OF THE TEST SPECIMEN.

GILLMORE TEST SPECIMEN.

Cement No.	Formation of Test Specimens.	Worked 1½ min.	Worked 3 min.
		Time of Initial Set.	Time of Initial Set.
D-2	1. Ball formed and top cut off; no troweling.	{ 4 : 30	4 : 42
		{ 4 : 32	4 : 45
	2. Standard Gillmore test specimens.	{ 4 : 40	4 : 42
		{ 4 : 41	4 : 45
	3. Standard pats; tops scraped with glass plate after forming to insure smooth surface.	{ 4 : 35	4 : 38
		{ 4 : 35	4 : 40

NOTE.—Normal consistency, 23.5 per cent, used.

TABLE X.—EFFECT OF NOT APPLYING THE NEEDLES SQUARELY UPON THE SURFACE OF THE TEST SPECIMEN.

Cement No.	Angle Made by Surface with Horizontal, deg.	Time Worked, min.	Time of Initial Set.		Time of Final Set.	
			(A).	(B).	(A).	(B).
D-2	6	1½	2 : 45	2 : 52
	6	4	2 : 11	2 : 17	4 : 26	4 : 46
	6	1½	2 : 43	2 : 50
	6	3	2 : 21	2 : 26
	8	1½	5 : 40	6 : 15
	10	4	4 : 37	5 : 11
	7	1½	5 : 36	6 : 04
	7	3	5 : 07	5 : 39
	8	1½	2 : 34	3 : 14	5 : 40	6 : 24
	13	4	2 : 19	3 : 08	4 : 33	5 : 21
	2	1½	2 : 35	2 : 39	5 : 28	5 : 32
	4	4	2 : 34	2 : 37	4 : 44	4 : 50
	5	1½	2 : 45	2 : 49	5 : 41	5 : 48
	5	4	2 : 28	2 : 38	4 : 37	4 : 56
	4a	1½	2 : 52	2 : 56	6 : 08	6 : 17
	3a	4	2 : 44	2 : 50	4 : 55	5 : 03

NOTE.—Normal consistency, 23.5 per cent, used, except (a), where 24.5 per cent was used.

(A) Needle applied perpendicular to surface of test specimen.

(B) Needle applied perpendicular to bottom of test specimen.

results shown for initial set by the Vicat method in the various tables also show the range of results which might be obtained unless care is used in trying several points in the mass.

GILLMORE VARIABLES.

9. *Effect of Additional Work in the Formation of the Test Specimen.*—The test specimens were formed as indicated in Table IX. The results indicate that the troweling as required by standard specifications has slight effect on the time of set of the cement mixed to normal consistency, and has the ad-

TABLE XI.—EFFECT OF VARIATIONS IN OBSERVING THE END-POINT INDENTATION OF NEEDLES.

GILLMORE TEST SPECIMEN.

Temperature, 28° C.

Cement No.	Group No.	Test Specimen Made by Observer	Relative Humidity, per cent.	Time of Initial Set.		
				Observer A.	Observer B.	Observer C.
D-2	1	A	90—100	3 : 54	3 : 46
		A	90—100	3 : 56	3 : 48
		A	50—52	2 : 30	2 : 21
		A	50—52	2 : 44	2 : 25
		C	90—100	3 : 44	3 : 44
		C	90—100	3 : 44	3 : 40
		C	50—52	2 : 45	2 : 22
		C	50—52	2 : 50	2 : 28

Temperature, 21° C.

D-2	2	A	90—100	5 : 05	5 : 09	5 : 02
		A	90—100	5 : 05	5 : 09	5 : 04
		A	40—42	3 : 57	3 : 54	3 : 37
		A	40—42	4 : 14	3 : 57	3 : 40
		B	90—100	5 : 17	5 : 12	5 : 05
		B	90—100	5 : 04	5 : 12	5 : 06
		B	40—42	3 : 43	3 : 42	3 : 37
		B	40—42	3 : 43	3 : 46	3 : 40
		C	90—100	5 : 23	5 : 08	5 : 07
		C	90—100	5 : 12	5 : 13	5 : 08
		C	40—42	3 : 57	3 : 53	3 : 37
		C	40—42	3 : 57	3 : 53	3 : 42

vantage of furnishing a smooth surface which aids in defining the indentation made by the needles.

10. *Effect of Not Applying the Needles Squarely upon the Surface of the Test Specimen.*—Test specimens were formed whose top surfaces were plane, but which made various angles with the surfaces of the glass plates on which they were molded. The different results were obtained by applying the needles perpendicular to the surfaces of the pats, and then perpendicular to the plates. The angle made with the horizontal in each case

is given with the results in Table X. For the slightly inclined surfaces which might normally occur in practice, the results obtained indicate that the error due to this factor is small.

11. *Effect of Variations in Observing the End-Point Indentation of Needles.*—To determine with accuracy the time when the hardening of the test specimen has reached the point where the needle makes no appreciable indentation requires that the pat have a smooth surface, be free from the small ridges and depressions sometimes caused by troweling, and that there be sufficient light. Together with the factors which have just been mentioned should be considered the personal equation of the observer. Table XI shows the variation in the results obtained by three observers on the same test specimens. The average of all the variations of the first group is nearly 13 minutes, and the average variation for the second group is nearly 12 minutes.

CONCLUSIONS.

Throughout this series of determinations an attempt was made to keep all conditions uniform except the one whose effect was to be noted. In practice, the results obtained on two consecutive days, may be affected by several factors which might combine to increase or decrease the range of values. For instance, a cool, damp day may be followed by a warmer day with a high relative humidity. The two factors on the first day both tend to retard the setting of the cement, while the high temperature of the second day, tending to shorten the time of setting, is opposed by the high humidity which reduces the amount of evaporation. To avoid the effects of these variables requires the use of a storage closet whose temperature and humidity can be controlled.

The variation in time of setting as determined by the same observer, thereby eliminating all errors due to personal equation introduced by several observers, is clearly shown in the various tables, and indicates that neither method will give results consistent enough to justify the reporting of results within the limits of a few minutes.

The other variables, such as formation of the test specimen and manipulation of apparatus, are of smaller importance, com-

pared with those of mixing and curing, but these errors may combine to increase those caused by the above.

The results obtained by varying the amount of work indicate that the test, as made at present, can be relied upon only to identify normal or slow-setting cements. The necessity for vigorous working in order that a normal cement may not be judged quick setting, defeats the object of the test when it is applied to a very quick-setting material, and may cause the set to be broken.

A study of the results makes it evident that neither the Gillmore or Vicat methods can be relied upon to give uniform results unless all factors which influence the rate of hardening are taken into account and controlled, and they further explain why comparative tests in a number of laboratories upon the same material have been found to give most variable, non-dependable results, cement often being adjudged quick setting in one laboratory and slow setting in another.

DISCUSSION.

Mr. Wig.

MR. R. J. WIG.—I regret that Mr. Williams is not here to present his paper in person. The work reported was not done to compare the relative efficiency of the two methods of determining the time of setting of cements, that is, the Vicat and Gillmore methods, but simply to determine the errors which may occur in using either of these methods. Many have found that if samples of cement are sent to a number of laboratories for testing, the results obtained showed great variation. The purpose of this investigation was to determine the causes for these variations.

Mr. Johnson.

MR. N. C. JOHNSON.—In Fig. 7 of Mr. Williams' paper, it is indicated that the center of the pat set more quickly than the outer portions. Is it not probable that this finding is in error, due to the effect of surface tension in resisting the penetration of the needle at the center? In some experiments on resistance of clays to penetration, it was found that this resistance was very marked, due to surface tension, so that for concordant results penetration had always to be measured at the center.

Mr. Goldbeck.

MR. A. T. GOLDBECK.—The results reported by Mr. Williams, in his tests to determine the possible errors which may occur in determining the time of setting of cement, must be of extreme interest to those who have to deal with the acceptance or rejection of this most important material, and they might tend to raise grave doubts regarding the applicability of present standard methods of testing to the performance of the functions for which they are intended.

It has long been known that such factors as temperature, humidity, degree of vigor in mixing, imperfections and lack of cleanliness in the instruments, carelessness in measuring and weighing, and personal equation, all enter into and control the final result, and thereby make it of necessity an approximate one. But when it is suggested that it is possible to obtain such results as are reported, when conscientiously following the letter

and spirit of the standard specifications, those concerned should not stop at mere interest, for the matter is one of great importance and deserves most careful attention. Mr. Goldbeck.

During the past year the writer has made some tests along much the same lines as those of Mr. Williams, and some of the results of these tests may be of interest since they throw additional light on the subject under discussion.

It might be well to consider in detail the effects of the variables cited by Mr. Williams.

1. *Effect of Slight Errors in Weighing the Cement and Measuring the Mixing Water.*—The maximum possible error in weighing the cement should not exceed 0.5 g. at any time, and this on a 500-g. sample equals 0.1 per cent and would be equivalent to from 0.02 to 0.03 per cent error in the mixing water. The maximum error in measuring the mixing water should certainly not exceed 1 cc. This in a 500-g. sample is an error of 0.2 per cent in normal consistency. It is shown in Table I of the paper that a range of 4 per cent in water causes a difference in time of setting of 107 minutes. Supposing it to be fair to accept these figures as truly representative, then the error for 0.2 per cent would equal 5.2 minutes as the maximum to be expected by carelessness in weighing and mixing. It hardly seems fair to accept these figures, however, since evidently the mixtures containing 21.5 and 25.5 per cent of water were not mixed to normal consistency, and time of setting tests are required to be made on normal mixtures.

2. *Effect of the Amount of Work done in Mixing the Cement.*—The effect of varying degrees of mixing is one of the recognized sources of error in the test for time of setting, but such results as reported in Table II of the paper are startling, if all of the conditions required by standard specifications were fulfilled. Was a different percentage of water used for the "less-vigorously" mixed batch than for the "vigorously" mixed batch? In the less-vigorously mixed batches was sufficient vigor used to obtain a final mix of smooth, uniform appearance? In the less-vigorously mixed batches was the cement really kneaded vigorously? Kneading implies that considerable pressure be exerted in the operation, and any one must recognize the necessity for vigorous kneading sufficient to obtain at least a smooth, seemingly

Mr. Goldbeck. uniformly mixed mass, through which the water is uniformly distributed. It must be asked if such was the condition of the less-vigorously mixed cements reported. The writer has made experiments in which he used five different operators, two of whom were men of years of experience in cement testing, one had at the most one week's intermittent practice, the fourth man was employed in chemical work entirely and had never before mixed cement, and the fifth had intermittent practice. Two of the men then were experts, two were novices and one had mediocre ability in cement mixing. All were required to determine their own normal consistency, and the time of setting was read by one man and checked by another. The results are given in Table I.

TABLE I.—DETERMINATION OF NORMAL CONSISTENCY AND INITIAL SET BY DIFFERENT OPERATORS.

Operator.	Normal Consistency; water, per cent.		Initial Set, hr. and min.	
	Vicat Method.	Ball Method.	Vicat Method.	Gillmore Method.
A (expert).....	21.75	22.5	3—1	3—53
B (medium).....	22.0	22.5	2—55	3—43
C (novice).....	22.5	23.0	3—29	3—54
D (expert).....	21.75	22.25	3—3	3—38
E (novice).....	22.25	23.0	3—29	4—18
Maximum Difference.....			0—34	0—40

NOTE.—Vicat method, 1 minute mixing, used for Vicat tests; ball method, 1½ minutes mixing, used for Gillmore tests. Temperature of room, 75° F.; temperature of damp closet, 74° F.

This is a very close agreement considering the experience of the different operators. The writer has tried to reduce the time of setting of different samples of cement mixed by the same person with different degrees of vigor, and thus far he has failed to obtain anything like the range of values reported. The results given in Table II are characteristic.

It will be noted that in the less-vigorously mixed batches, in which the same percentage of water was used as required for normal consistency when vigorously mixed, the mixture was too

dry, as is indicated by the small penetration of the Vicat plunger. Mr. Goldbeck. The results for time of setting, however, are not greatly different from those in the vigorously mixed batches. In order to make the less-vigorously mixed batches of normal consistency, higher percentages of water were added, and the effect was to lengthen the time of initial set by about one hour. It is the writer's opinion that the character of the mixing performed in the less-vigorously mixed batches could not possibly be construed as vigorous mixing by any operator accustomed to testing cement, since the mixing was not even vigorous enough to produce a smooth uniform mass.

TABLE II.—EFFECT OF DEGREE OF VIGOR OF MIXING ON TIME OF SETTING.

Cement No.	How Worked.	Percentage of Mixing Water.	Penetration of Vicat Plunger, mm.	Initial set, hr. and min.	
				Vicat Method.	Gillmore Method.
1	Vigorously.....	23.0	10½	2-30	3-0
	Less vigorously.....	23.0	8	2-57	3-22
	Less vigorously.....	25.0	10	2-55
2	Vigorously.....	22.0	10	2-50	3-45
	Less vigorously.....	22.0	3	2-35	3-32
	Less vigorously.....	23.5	11	3-50	4-22
3	Vigorously.....	22.0	9½	2-39	3-47
	Less vigorously.....	22.0	3	2-37	3-27
	Less vigorously.....	23.5	7	3-13	4-3
	Less vigorously.....	24.0	12	3-15	4-30
	Less vigorously.....	23.75	11	3-31	3-59

The effect of an increase in the time of mixing in shortening the time of setting corresponds with results obtained by the writer.

Tables IV and V of the paper are interesting, since they show the small effect of small differences in humidity on the time of setting. The Vicat specimens seem to be affected to no definite extent, whereas the Gillmore specimens are slightly affected.

The author's reference to the variables which affect the results obtained with the Vicat needles and the Gillmore needles deserves attention. Considering in order the Vicat variables cited:

Mr. Goldbeck.

5. *Effect of Additional Work in the Formation of the Test Specimen.*—This of course is unavoidable, and is common to both the Vicat specimen and the Gillmore specimen, with the exception, however, that this additional work is practically a constant quantity in the Vicat specimen, but may be extremely variable in the Gillmore specimen.

6. *Effect of Dirty and Bent Needles.*—A dirty needle is inexcusable and can always be avoided. Any appreciable bend in the needle can always be detected after the first trial penetration by the shape of the hole formed and may be immediately remedied for future trials. Errors due to these causes, therefore, should never exist.

7. *Effect of Releasing the Needle Above the Surface of the Test Specimen.*—If the needle is released when not exactly in contact with the test specimen, the specifications are not being followed and this error is therefore inexcusable.

8. *Effect of Lack of Homogeneity of the Mass and Variation in Observing the Indentation of the Needle.*—Unfortunately there always seems to be a lack of homogeneity in the mass, which accounts for the range obtained between the first and final reading. However, in operating the Vicat needle very close results can be obtained by observing the penetrations in a number of points equally distanced from the center and judging of the set from these readings. Regarding the variation in observing the indentation of the needle, the writer fails to see how there can be any variation at all, as the scale readings may be taken so definitely.

The author in Table VII shows the effect of heaping the cement above the Vicat ring and then troweling the top smooth. Of course an error in time of setting is produced in this way, but it is inconceivable to the writer why such an error should even be mentioned. Standard specifications are very explicit on the method of forming the test specimen; they were made explicit in order to avoid this very error, and errors produced through the practice of wrong methods are always avoidable. Additional work in the formation of the Vicat specimen does not cause an error when specifications are followed, and should not be considered as one of the variables in the Vicat method.

The author mentions the impossibility of obtaining a flat

surface on the Vicat specimen when following specifications. **Mr. Goldbeck.** This has not been the writer's experience. An experienced operator, by following specifications, can produce a surface practically all of which is level, or at least so nearly level that the time of setting is not thereby affected.

The following Gillmore variables are cited by Mr. Williams:

9. *Effect of Additional Work in the Formation of the Test Specimen.*—This effect probably in general does not cause any very great error, although in the formation of the pat used, a variable amount of troweling may be performed by different operators.

10. *Effect of Not Applying the Needles Squarely upon the Surface of the Test Specimen.*—This could probably be obviated if the needles were always mounted and great care were exercised in troweling the top of the specimen as plane and parallel to the base as possible.

11. *Effect of Variation in Observing the End-Point Indentation of Needles.*—This is an objectionable feature of the Gillmore needle and subjects the results obtained to the operator's personal equation, so that an unavoidable error is thereby produced.

MR. RICHARD L. HUMPHREY.—I think in considering the **Mr. Humphrey.** results of the tests reported by Mr. Williams, we should remember that the requirements of the standard methods of tests should be followed in their entirety. I believe, therefore, that it is not possible for anyone to test cement according to the present prescribed methods of the Society, and get the wide range of values reported. This is indicated in the experiments presented in Mr. Goldbeck's discussion. I think it is admitted by everyone that there are a great many variables entering into the determination of time of setting, and that the method is at best only an approximation. Anyone can interpret "vigorously mixing" as not permitting a mild mixing with the fingers, without the exertion of pressure; and it certainly is a fact that to obtain the normal consistency required by the standard specifications of the Society, vigorous mixing is necessary. Cement when so mixed will certainly not show the range in setting time that has been reported. These tests ought to be repeated before accepting the conclusions which they seem to justify.

Mr. Hasse.

MR. O. C. HASSE.—It came to my observation recently that cement seems to set differently in different places; in other words, geographic location seems to have some effect upon the setting time. This may stimulate some research along this line, and I simply offer this as a suggestion.

Mr. Wig.

MR. WIG.—No difference or change in the temperature of the mass in the test piece could be detected by inserting a thermometer in the cement used. There may have been a slight change in temperature, but we do not believe it was sufficient to cause this difference in penetration, although it may have been.

In regard to Mr. Goldbeck's criticism, I think we have been hoodwinking ourselves into believing that we get accurate results, because we have in the Vicat apparatus a needle with a millimeter scale. Tests on the same cement in different laboratories, where they knew they were making check tests, showed variations of 500 or 600 per cent, and this occurring in the best laboratories of the country is pretty good evidence that the method is unsatisfactory. This investigation was made to determine the effect of each variable on the time of setting.

In regard to the likelihood of the operator mixing cement differently for the normal consistency test and for the time of setting test, I would suggest that in many large laboratories one operator determines the normal consistency, and then several operators make up the test pieces, and they all differ in the manner and speed with which they work. I believe, therefore, that the results reported in this paper are not unreasonably extreme. Furthermore, it would be a simple matter to repeat these tests in your own laboratory.

Mr. Williams.

MR. G. M. WILLIAMS (*Author's closure, by letter*).—It is not claimed in the paper that any new factors affecting the time of setting of cement have been discovered, but the results are given to show the relative importance of these factors and the range of values which may be introduced by them, at the same time keeping the requirements of the specifications in mind.

It is possible that the penetration of the needle is slightly affected by surface tension immediately after the test piece is molded and while the mixture is still very plastic. However, after a period of time the whole mass stiffens, and it is not reasonable to suppose that the surface tension has a greater effect

than the hardening interior of the mass. This is well shown by Mr. Williams. the fact that the needle may penetrate slightly, then cease to move, and suddenly drop downward, indicating that there is resistance in the mass below the point of the needle which must be overcome. A mass of clay is undergoing no chemical change while there is the chemical action in a mass of cement which brings about the hardening effect, so that the two cases are not exactly similar.

In regard to variation in setting time in different geographic locations, it is possible that the results are due, at times, to changes in the cement itself, but more commonly due to variations in temperature and humidity which would be found in different geographic locations, especially if the test pieces were exposed to the room atmosphere, as is done in some laboratories.

While an error of 0.5 g. in a 500-g. sample is theoretically the greatest error which should be allowable, anyone with experience knows that the error is more likely to be one of 5, 10, or 20 g., due to the use of wrong weights at times, by even the most careful operators.

Referring to lack of homogeneity of the mass in paragraph 8 of Mr. Goldbeck's discussion: In case uniform results may be obtained by taking observations in a certain section of the Vicat test piece, it seems that this uniform zone should be carefully described in the specifications in order that operators in different laboratories may know where to find it, since they have not been able to locate it unaided, judging from the results obtained in various laboratories.

Throughout the various tests of cement, the question of personal equation of the operators and the conditions under which they work must be considered: In some cases tests are made in small, poorly equipped and arranged rooms where there is small incentive to careful work. The testing is often entrusted to boys, who, though experienced, have little personal interest in the reliability of their results, and may disregard the requirements of the specifications.

The "variations in observing the indentation of the needle" applies to the determination of the final setting time of the Vicat test piece.

Mr. Williams. That opinions will differ as to the exact meaning of the words of a specification is to be expected, and from definition of the term, "vigorous mixing" is certainly dependent upon the judgment of the individual operator. The amount of work may vary from that amount defined in the paper as "less vigorously" to that amount which would satisfy the most critical operator.

As was stated in the paper, the plasticity of the mass was increased by additional work, or more vigorous work, up to a certain point, but it is no more to be expected that the same condition could be reached by two operators working in different laboratories than that they could be expected to check each other's results for normal consistency, which tests show is not done.

Whatever factor or factors in practice are responsible for the great variations found in time-of-setting results, the fact remains that the range of values may be as much as several hundred per cent among different laboratories, and it seems reasonable to consider that the variation is due to those factors which have shown the greatest variable results in the preceding discussion.

VOLUME CHANGES IN PORTLAND CEMENT AND CONCRETE.

By A. H. WHITE.

SUMMARY

Portland-cement structures retain their colloidal property of expanding when wetted and contracting when dried, even after 20 years' exposure to the weather, and after baking to 600° F. A bar of neat cement may be made to progressively expand or contract according to the choice of moisture conditions. It may be made by successive bakings to contract as much as 4.6 per cent. Cement bars containing sand do not contract readily on account of the interlocking of the aggregates. When such bars are dried they become more porous and, therefore, on immersion, more water can enter, so that new particles of cement clinker will be hydrated, with a still further resultant elongation of the bar. Bars of 1:3 sand mortar have by successive bakings and boilings been made to elongate 0.75 per cent. Bars cut from sidewalks taken up after 20 years' service have been made to elongate 0.175 per cent by successive immersions in water at room temperature. This elongation is as much as would be calculated for a rise in temperature of 300° F. It must be considered a grave source of danger to concrete highways in wet locations. Dense concrete, even when wet, does not contain any liquid water and does not suffer any expansion on freezing. Such concrete when measured while frozen will be found to have contracted at the same rate as cast iron. Porous concrete containing liquid water expands on freezing and does not return to its original length on thawing because of the wedging of the aggregates. Repeated freezings will thus cause progressive disintegration.

VOLUME CHANGES IN PORTLAND CEMENT AND CONCRETE.

By A. H. WHITE.

Three years ago the author¹ presented a paper before the Society in which he showed that Portland cement after hardening in water still undergoes very marked changes in volume as it becomes alternately wet and dry. Experiments during the last three years have confirmed the earlier results and shown that the destructive action of these forces of expansion and contraction may be even more serious than was thought probable when the first paper was published. The present paper is a continuation of the preceding, and discusses not only the effect of varying moisture but also of high and low temperatures.

To understand these phenomena it is necessary to consider what happens when Portland cement hardens. The elder Michaelis² set forth in 1893 his colloidal theory of the hardening of Portland cement which now finds general acceptance. In accordance with this theory Portland cement reacting with water swells to a gelatinous colloid, which cements together the particles of sand and gravel in much the way that glue would. Each granule of cement thus surrounds itself with a colloidal envelop of progressively increasing thickness and density, through which water forces its way with greater and greater difficulty, until finally the reaction stops because no more water can gain access to the still unattacked kernel of cement.

Colloids may be divided into the two classes of reversible and irreversible. Glue is a reversible colloid in that when again moistened it swells and becomes soft. Clay is also a reversible colloid so long as it is not baked. When baked to a hard brick it is no longer reversible and is practically unaffected by water. Although the theory that hardened Portland cement is a colloid

¹ *Proceedings, Am. Soc. Test. Mats.*, Vol. XI, p. 531 (1911).

² *Chemiker Zeitung*, Vol. 17, No. 2, p. 1243 (1893).

is generally accepted, the assumption has been that it is an irreversible colloid, and that when once formed it is stable and permanent to the same degree that a burned brick is stable. It was the privilege of the author in his previous paper on this subject to show that this assumption is erroneous and that cement products, after hardening under water, shrink when they become dry and expand when they become wet, and repeat this process over and over as conditions alternate. In the present paper a more exact study is made of this subject, and an explanation is given of the progressive disintegration of concrete which may take place under certain conditions of alternate exposure to water and air.

The relative magnitude and importance of the changes in volume due to moisture may be best understood by comparison with expansion due to temperature. The usual figure for the expansion of concrete is 0.000055 or 0.00055 per cent for each degree Fahrenheit change in temperature. The changes in volume due to moisture as shown in this paper are usually more than one hundred times as great as this, and are sometimes five hundred times as great. The experimental errors of measurement are therefore relatively small.

The figures quoted in this paper are from measurements on bars about 4 in. long and 1 in. square, measured with the micrometer devised by Campbell¹ for this purpose. The probable error of reading this instrument is 0.003 per cent on bars which have been cast with glass plates embedded in the ends. When bars are sawed from concrete blocks and the measurement must be made directly on the concrete, the error is somewhat greater on account of the irregularities of the concrete surface. The probable error of the micrometer measurements is therefore equivalent to a temperature change of about 6° F.

EFFECT OF VARIATION IN MOISTURE ON NEAT CEMENT.

The paper presented three years ago traced the changes in length of several neat bars made from different brands of Portland cement, and showed that after 1 year in water they came to approximately constant volume, and did not suffer material

¹ *Journal, Am. Chem. Soc.*, Vol. 28, p. 1276 (1906).

expansion during 2 years' further immersion, but that when dried in air at room temperature they contracted to nearly their initial volume and expanded again on being placed in water. The results of 3 years' further study of these bars together with the earlier records is shown graphically in Fig. 1. Bar No. 131 A is of Rosendale cement. The other four are Portlands, bar No. 131 C being of Atlas, No. 131 E of Omega, No. 131 G of Universal, and No. 131 I of Peninsular. A wide variety of types of commercial cements is thus included, but the results are so similar in the case of the Portland cements that the curves can all be treated alike. The values for the expansion of the bars during the first year in water differ somewhat from each other, but this difference may well be due to differing moisture conditions at the time of the initial measurements and not to inherent differences in the cement itself.

When a bar of neat cement is removed from water and placed in air it shrinks in length for about 60 days, and then fluctuates slowly with the humidity of the air. When placed in water again it expands rather rapidly for 1 week, and then at a decreasing rate for 1 month, the total change in length in this process being about 0.15 per cent. The bars shown in Fig. 1 have been carried through four complete wet-and-dry cycles during the last 3 years, each cycle showing almost the same changes in volume. An examination of Fig. 1 shows, however, that the bars are gaining in length each time they are allowed to complete their reaction with water. Two of these bars are 0.08 per cent longer than they were after they had lain in water for the first 3 years of their existence. This systematic growth with alternate wetting and drying has an extremely important practical bearing, and is discussed more fully under the heading, "Effects of Systematic Changes in Moisture."

The Rosendale cement, No. 131 A, differs from the four Portland cements only in the degree of the changes. The expansion during the first 3 years' immersion in water was three times as great as that of the Portland cements, and the change in length during the first wet-and-dry cycle was also much greater. The differences during the last 3 years are about the same as for Portland cements.

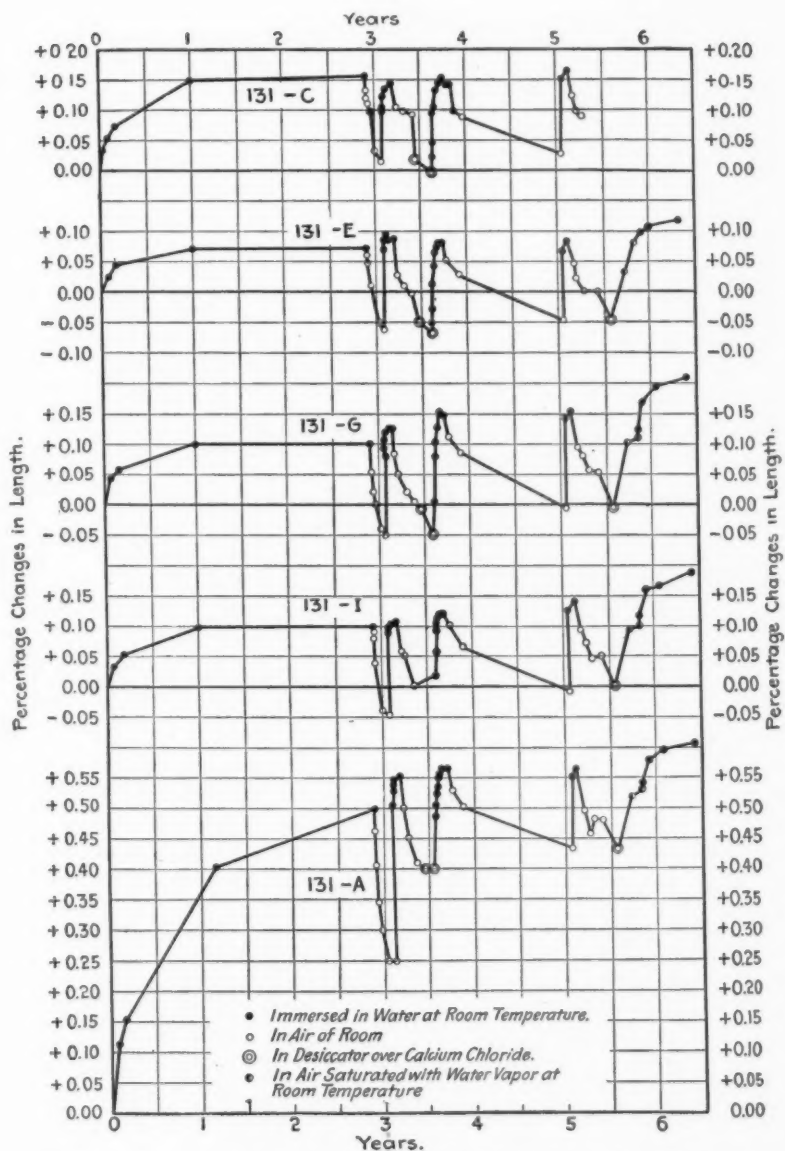


FIG. 1.—Changes in Length of Neat Cement Bars when Alternately Wet and Dried at Room Temperature.

PROGRESSIVE ELONGATION OR CONTRACTION CAUSED BY
SYSTEMATIC CHANGES IN MOISTURE.

In the above experiments the bars were kept long enough under water and in dry air to allow the reaction to come practically to a standstill, with the result that the bars became pro-

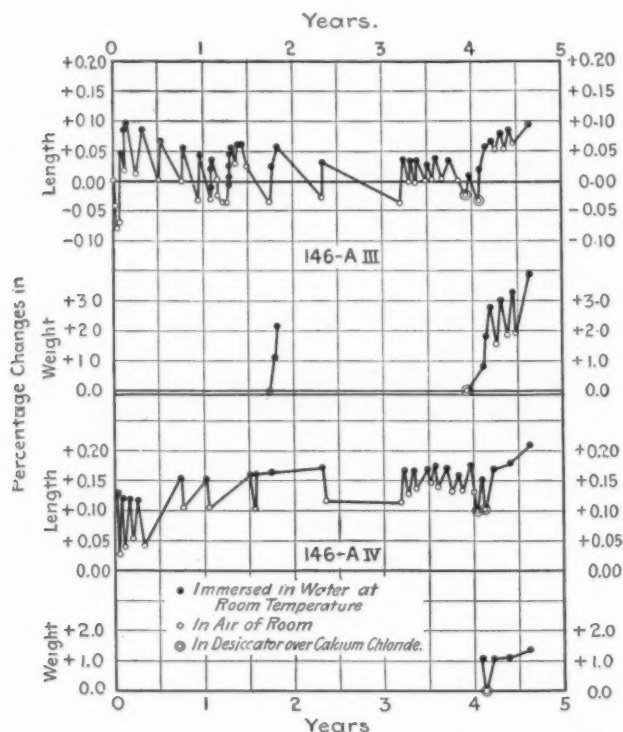


FIG. 2.—Progressive Changes in Neat Cement Bars when Alternately Wet and Dried at Room Temperature.

gressively longer. Two bars, Nos. 146 A, III and IV, were made up as duplicates to test the effect of systematic variation of duration of exposure to water and air, and it was found that by suitable manipulation the bars could be made to increase or decrease in the mean length, about which they fluctuated with changing moisture conditions.

One of the bars, No. 146 A, III, was kept in air for the first 30 days of its existence, and was then alternated between air and water according to a system which in general kept it dry for periods of 2 months or more at a time, and wet for shorter periods of only a week. The result was that the length of this bar, as shown in Fig. 2, dropped during the first 3 years from a mean of $+0.05$ per cent which it showed in the second month to practically its initial length. During the fourth year the time of exposure to air was shortened to less than a month so that the bar did not have a chance to fully dry out, and the mean length rose slightly. At the end of the fourth

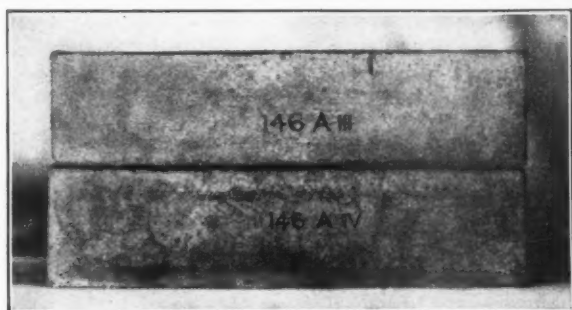


FIG. 3.—Effect on Duplicate Neat Cement Bars of Systematic Variation in Moisture Conditions in Room Temperature for Four Years. The Life Histories of these Bars are shown in Fig. 2.

year it was dried thoroughly in a desiccator and soaked in water the usual time when its mean length dropped below the zero. At the beginning of the fifth year it was decided to try and build this bar up to the length of its duplicate and accordingly it was given longer immersions in water and shorter exposures to air. The effect is evident from an inspection of the curves which show that after 6 months of such treatment the bar is alternating about a mean length of $+0.08$ per cent instead of about a mean length of zero, and is growing in length with each successive treatment.

The duplicate bar, No. 146 A, IV, was left in water instead of air during the first 30 days after removal from the mold, and was then alternated on a schedule which during its first two

years gave it relatively long periods in water and short periods in air. Its mean length increased rapidly in the first few months under this treatment, and then more slowly until it stood at about $+0.14$ per cent. A prolonged drying for nearly a year failed to bring its length below $+0.11$ per cent, and subsequent treatment with water caused it to show slightly greater expansion than before.

At the end of 4 years' treatment, one of these bars after thorough drying in a desiccator over calcium chloride had a length of -0.029 per cent, while the other showed $+0.100$ per cent. This difference is reflected in the appearance of the bars which is shown in Fig. 3. The bar No. 146 A, IV, showing the permanent expansion, is covered with a distinct network of hair cracks, while the other bar is almost entirely free from them.

EXPLANATION OF PROGRESSIVE VOLUME CHANGES.

The colloid theory of the hydration of Portland cement affords an explanation of the alternate expansion and contraction of Portland cement as it becomes alternately wet and dry, but it requires something additional to explain the progressive expansion shown by the five bars of Fig. 1 and by bar No. 146 A, IV, of Fig. 2.

A bar of neat cement is composed of particles of unchanged cement clinker embedded in a colloid. Only a small amount of the clinker becomes hydrated before the swollen colloid fills all the interstitial spaces and thereby expands the bar. The colloid ultimately becomes so dense that it shuts off further action of water. So long as this condition persists the volume of the bar remains constant. If the bar becomes dried the colloid shrinks and the bar contracts, but usually not to its initial length. It may be assumed that some of the clinker particles have become wedged in their new positions and do not easily return to those formerly held. The bar is thus more porous than formerly, and when again immersed a larger amount of water can enter and form colloid with unchanged clinker, with the result that the bar undergoes still further expansion. This increase might take place with each cycle until ultimately all the cement clinker had become hydrated. The expansion thus

produced should be permanent except in a young bar, as shown in bar No. 146 A, III, for although the colloid thus produced will shrink on drying, it will not under ordinary conditions shrink to the volume of the clinker from which it was made. Confirmation of this is given by bar No. 146 A, IV, which after a 1-year drying in air and again after drying over calcium chloride, refused to contract below +0.10 per cent. The progressive increase in length of the dried bars in Fig. 1 tells the same story, and various bars reported further on in this paper confirm the conclusion.

The correctness of this explanation of progressive volume changes is also confirmed by studies in change of weights and

TABLE I.—CHANGES IN LENGTH, WEIGHT AND DENSITY OF A DRY BAR OF NEAT CEMENT (No. 146 A, IV) ON SOAKING IN WATER.

Condition of Bar.	Change from Initial Measurement, per cent.	Change from Dry Weight, per cent.	Specific Gravity.
Dried over calcium chloride.....	+0.100	0.00
After $\frac{1}{2}$ hour in water in vacuum.....	+0.097	0.13	2.310
After 2 days in water.....	+0.117	0.41	2.314
After 3 days in water.....	+0.121	0.46	2.315
After 1 month in water.....	+0.171	1.12	2.325
After 2 months in water.....	+0.176	1.28	2.327
After 3 months in water.....	+0.186	1.37	2.330
After 6 months in water.....	+0.214	1.46	2.336

density. Unfortunately, the importance of these figures was not realized until recently, and so figures representing changes during the early life of these bars are not available. There is an error in weighing a wet bar on account of the variability of surface moisture, which amounts to about 0.05 per cent and causes an error in density of about 0.002. Bar No. 146 A, IV, when 4 years old, was dried thoroughly in a desiccator over calcium chloride and then immersed in water in a vacuum for $\frac{1}{2}$ hour to pump out the occluded air. It was then kept in water at room temperature for 6 months. The changes are shown in Table I.

It will be noted that this 4-year-old bar of neat cement, although it had been dried in a desiccator for over 1 month,

absorbed almost no water on being immersed for $\frac{1}{2}$ hour in water in a vacuum, indicating an absence of voids; and that it actually contracted in length during this $\frac{1}{2}$ -hour immersion. Nevertheless, on lying in water it slowly gained in length, weight and density. We have here the unusual phenomenon of a body more than twice as heavy as water absorbing water and still increasing in density in the process. The liquid water which is being absorbed must undergo conversion into some other form with great decrease in volume. The compression deformations involved in this process cannot even be guessed at, for we have no exact knowledge of the form which the water assumes when it reacts with the cement.

This increase in density of cement bars as they lie in water has been shown by every bar tested, but not enough data are at hand to allow any generalization as to its practical bearing. Nor do the relative changes in weight and volume of one bar correspond with another. The companion bar (No. 146 A, III) to the preceding after thorough drying was immersed in water in a vacuum for 4 hours. It increased in weight 0.3 per cent and in length 0.005 per cent. On lying in water for 2 months, it increased in weight a total of 2.79 per cent and in length 0.101 per cent. This latter bar showed more than twice the percentage gain in weight that its companion, No. 146 A, III, indicated, but only one-third greater gain in length. Other figures showing changes in weight and density will be presented later in the paper.

These experiments have shown that it is possible by suitable moisture conditions to cause progressive expansion in cement bars 5 years old, and that though progressive contraction may be brought about in young bars, it is possible to bring about a progressive shrinkage only to a very limited extent with older bars. The experiments on old sidewalk units described further on in this paper indicate that the limit of expansion has not been reached after 20 years' exposure to weather.

ATMOSPHERIC HUMIDITY AS A CAUSE OF CHANGE OF VOLUME.

Irregularities in measurements made during the summer time on bars, which should presumably have come to constant

length in the air of the room, aroused the suspicion that the humidity of the air was affecting their length. To test this, the bars after being thoroughly dried over calcium chloride were placed in air saturated with water vapor at room temperature. They expanded slowly and after 3 months the bars of neat cement had lengthened about 0.10 per cent, which is rather more than half the elongation which they later attained when immersed in water. The changes in four bars of neat cement are shown graphically in Fig. 1, where toward the end of the fifth year on each curve will be found two circles, half-black and half-white, indicating measurements made after the bars had been in damp air.

This same phenomenon has been observed by Jesser¹, who made bars of neat cement and allowed them to lie in water for 30 days before removal to air of controlled moisture content. He found that if the air was about one-third saturated with moisture the contraction was approximately 0.15 per cent, if one-half saturated with moisture it was 0.14 per cent, and if two-thirds saturated it was 0.10 per cent. Bars made from one part of cement and three of sand changed approximately one-fourth as much as those made from neat cement. Jesser's measurements were made on bars only a few weeks old, while those reported in this paper are on bars nearly 6 years old. Nevertheless, the two sets of experiments show substantially the same result, and indicate that the humidity of the air may cause a distinctly greater expansion than temperature. The effects of temperature and humidity will frequently be additive, since it is in hot weather that the humidity is also high.

EFFECT OF VARIATION IN MOISTURE ON 1:3 SAND MORTAR BARS.

On the assumption that any changes in bars of 1:3 sand mortar are due to the cement, it would be expected that the curves showing variation of length of the mortar bars with moisture would vary in the same manner, but to a less degree than those showing changes of neat cement. It might be ex-

¹ "Einfluss der Wasserdampfension der Luft auf das Volumen des Cementmörtels," published by Cement Verlag, Charlottenburg, 1913.

pected that the changes would be extremely small since the voids in the mortar would have to be filled by the expanding colloid before any expansion would be evident.

The curves of Fig. 4 show that the changes are indeed in the same direction as those found with neat cement. The changes are, however, larger than might have been expected from the proportion of cement, and average 0.06 per cent in the last change from the wet to the dry state. The expansion of these bars in damp air is about one-half this amount. The mean length of these mortar bars is in most cases less than their initial measurements. They have, however, by accident rather than by intention, all been kept dry for much longer periods than they have been immersed in water except bar No. 146 X, which has been given very few alternations. It is planned to carry on the tests from this point so as to keep the bars wet for long periods and dry for short ones, to see whether they cannot be made to grow as the bars of neat cement did.

EFFECTS OF VARIATIONS IN MOISTURE ON COMPOUND BARS.

In order to study the differential expansion and contraction occurring when cement mortars of different richness are bonded together, a series of compound bars was made by casting a bar of neat cement 1 in. square on a bar of freshly made 1:3 sand mortar of similar dimensions. The tests of four of these bars during their first 17 months were presented in the earlier paper. The life history of these bars for their first 4 years is given in Fig. 5. The neat portion of the bar changes its length from 0.10 to 0.15 per cent, as it is alternately wet and dried at constant temperature. The 1:3 sand mortar portion of the bar changes about 0.05 per cent in length under the same conditions. Bars Nos. 146 II and 146 VII were subjected to tests at higher temperatures when nearly 4 years old, and their history from that point is discussed elsewhere in this paper. Bars Nos. 146 VI and 146 I near the end of their fourth year were exposed to air saturated with water vapor at room temperature. The neat portion of each bar increased in length 0.07 and 0.08 per cent, respectively, after 3 months in damp air, over that which they had attained after standing in dried air. On immersing in water at the end of this period they gained only 0.04 per cent more.

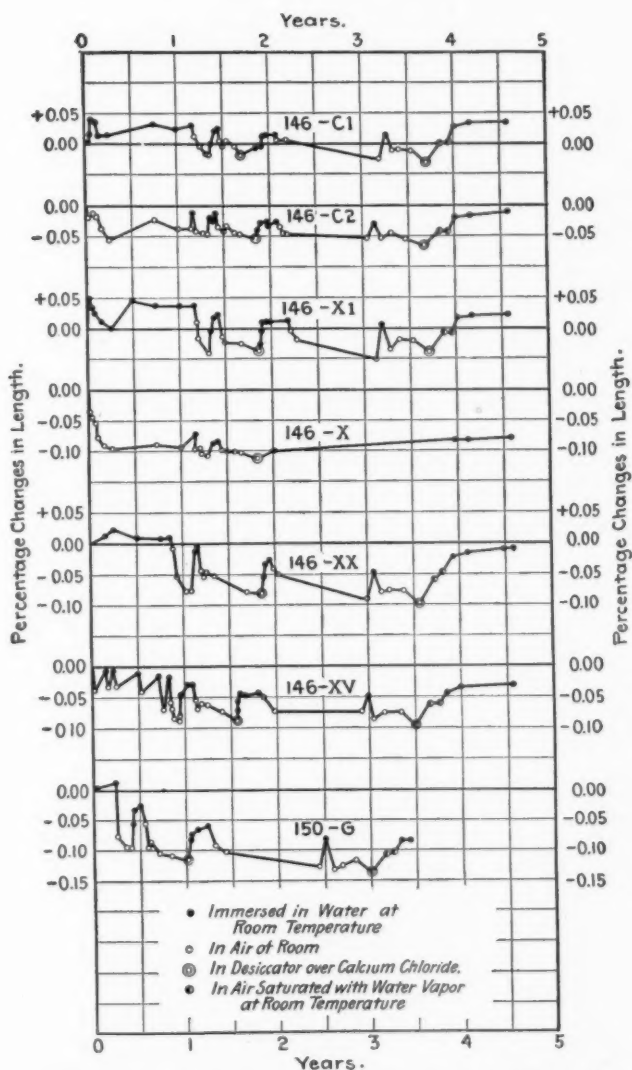


FIG. 4.—Changes in Length of 1:3 Sand Mortar Bars when Alternately Wet and Dried at Room Temperature.

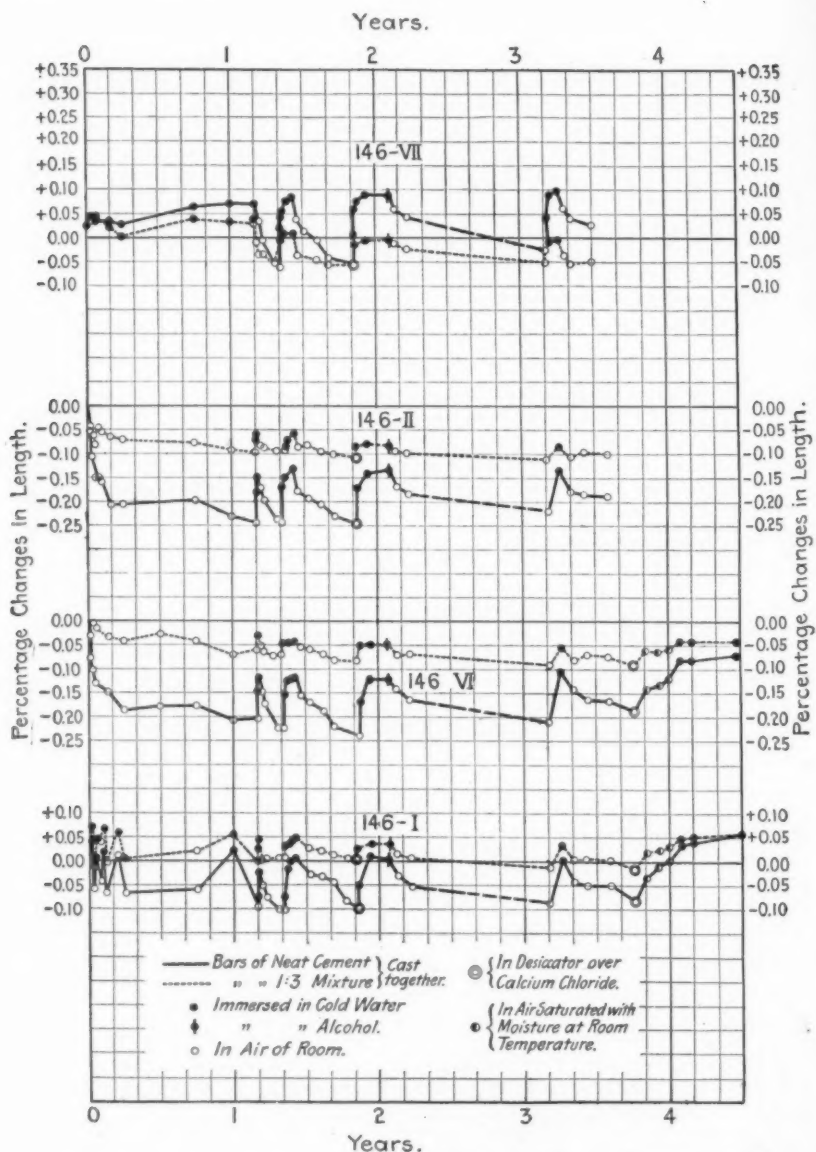


FIG. 5.—Changes in Length of Compound Bars when Alternately Wet and Dried at Room Temperature.

Both the neat and mortar portions of the bars expanded two-thirds as much in damp air as in water.

These four bars were made from the same brand of cement substantially as duplicates of each other to test the effect of differing moisture conditions. The top bar, No. 146 VII, shown in the upper curve of Fig. 5, was kept in water during the first 14 months of its life. This gave good opportunity for the development of colloid, so that its subsequent changes of volume on wetting and drying were the largest of the set. The neat portion of the bar was always longer than the sand and was therefore in compression. Especial attention is called to this point, since by exposure to higher temperatures the mortar portion was subsequently made to expand unduly as described later, and so put the neat portion in tension. The neat portion then cracked. So far as the curve of Fig. 5 shows, however, the mean length of this bar was not far different from its initial measurement, and therefore it remained in good condition. Bars Nos. 146 II and 146 VI were dry during their first 14 months, so that the neat portion contracted decidedly more than the mortar portion and was thrown into tension. Hair cracks appeared in the neat bar and extended progressively till they reached the sand portion. Bar No. 146 I was kept alternately wet and dry in such a manner that its mean length was not far from its initial measurements. It shows almost no hair cracks and is in good condition. The value of these curves lies in their illustration of changing stresses which may be produced in non-homogeneous structures through changing moisture content. Even alteration in the humidity of the air may cause material changes.

STUDIES OF OLD SIDEWALK UNITS.

In July, 1910, a sidewalk was taken up on the University of Michigan campus to make room for a new building. This sidewalk had been laid about 20 years previously by university workmen, using imported German cement, and was still in an excellent condition. No specifications have been preserved for this walk, but it was made with a rich upper coat and a lean base containing gravel up to the size of a hickory nut. Bars about 4 in. long and 1 in. square were sawed from this walk and observed in the laboratory. Bar No. 156 B contains a portion

of both top and base; bar No. 156 C is top alone and bar No. 156 D is the adjacent portion of base. The changes in weight and length of these bars while alternately wet and dried at room temperatures are shown in Fig. 6. The measurements of the bar made when thoroughly dry are taken as the zero of the curves.

Tests during the first 6 months showed a change in length of the top portion of from 0.05 to 0.07 per cent as it was wet and dried. The base showed practically the same change in length—an unexpected result on account of the leanness of the base. The bars were now thoroughly dried over calcium chloride and then placed in water. They were measured four times during the first month and then were allowed to lie entirely undisturbed for 2 years.

TABLE II.—PERCENTAGE INCREASE IN WEIGHT AND VOLUME OF OLD SIDE-WALK UNITS WHILE SOAKING IN WATER.

Description.	After 24 Hours.		After 1 Week.		After 3 Weeks.		After 4 Weeks.		After 2 Years.	
	Length.	Weight.	Length.	Weight.	Length.	Weight.	Length.	Weight.	Length.	Weight.
No. 156 C, top coat.....	0.023	1.1	0.055	2.1	0.062	2.8	0.064	...	0.163	3.4
No. 156 B, { top coat.....	0.035	4.8	0.060	...	0.112	5.0	0.167	...	0.208	6.3
base.....	0.047		0.057		0.062		0.068		0.137	
No. 156 D, base.....	0.032	4.9	0.050	5.5	0.050	6.0	0.500	...	0.120	8.1

The changes throughout the life of these bars are shown graphically in Fig. 6, and their increase at selected points during this 2 years' soaking is given in Table II.

The increase in length of the dry bars after being soaked in water for 24 hours was reasonably uniform, and may probably be ascribed at least in part to capillary action of the water. The base increased in weight much more than the top coat during this first 24 hours, as was to be expected from its greater percentage of voids. During the next 3 weeks, the length of the rich top coat increased more rapidly than the leaner base, as would have been predicted on account of its higher percentage of cement. The top coat of the compound bar had expanded especially rapidly, so that it was 0.167 per cent longer after

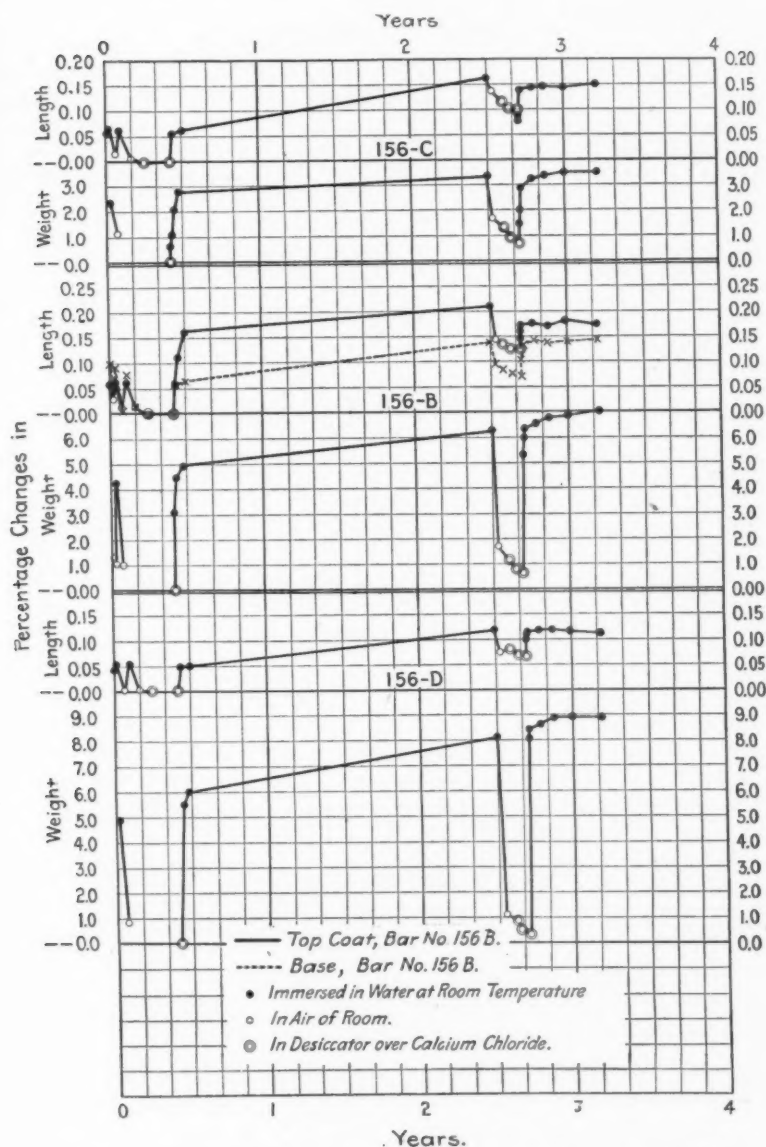


FIG. 6.—Changes in Length and Weight of Old Sidewalk Units when Alternately Wet and Dried at Room Temperature.

4 weeks in water than when dry. These bars were then left undisturbed for 2 years and when again examined were found to have expanded still more. Even the lean base of the walk had become 0.120 per cent longer as measured on one piece, and 0.137 per cent longer as measured on another. It was not a surprise to find that the rich top coat had expanded to an average figure of 0.18 per cent, but the high elongation of the base was unexpected. The duplicate results make it improbable that the error was one of measurement and the accuracy of the work is further confirmed by an inspection of the figures showing change of weight.

Comparing the figures for 3 weeks' immersion in water with those taken 2 years later, we find that the weight of the bar sawed from the top coat increased only 0.6 per cent in the 2 years, while the weight of the bar made from the base increased 2.1 per cent. The increase cannot have been in the weight of the gravel and can only have come from the cement. The explanation is simple in the light of the preceding discussion. The bar from the rich top coat with its high cement content, absorbed water rapidly until all of the capillary openings were filled with a dense colloid through which water penetrated very slowly if at all. The lean base with its large volume of voids allowed a greater proportion of its cement to react with the water. Until the voids were filled expansion was slight. When these were filled, the pressure of the cement still hydrating rose to the same possible maximum as in a richer mixture, or in neat cement. The results were unexpected but are explicable.

These bars after 2 years' immersion in water were placed in air, and subsequently in air dried with calcium chloride. All decreased in length and weight, but at the end of 2 months' drying had not yet returned to the figures shown in the dry state 2 years before. The weights of the bars were nearly those in their former dried condition, but the lengths were decidedly greater. Roughly speaking, their shrinkage was only half their previous expansion, so that they retained an apparently permanent expansion which averaged 0.11 per cent for the top coat and 0.07 per cent for the base. This is also in agreement with experiments previously described and is laid to the interlocking of the particles of aggregate.

On placing these bars again in water they expanded as before. The change during immersion in water was followed more closely than during the previous immersion, the weight of the bars being observed when suspended in water as well as when in air, so that changes in density could be observed. The air in the pores of the bars was removed rapidly when the bars were first immersed in water by a vacuum pump. After $\frac{1}{2}$ hour of this treatment, measurements were made which represented the changes due to the capillary action of the water. Subsequent changes could be laid only to chemical action of the water.

TABLE III.—CHANGES IN LENGTH, WEIGHT AND SPECIFIC GRAVITY OF OLD SIDEWALK UNITS ON SECOND IMMERSION.

Condition.	No. 156 B, Compound Bar.				No. 156 C, Top Coat.			No. 156 D, Base.		
	Change in Length, per cent.		Change in Weight, per cent.	Specific Gravity.	Change in Length, per cent.	Change in Weight, per cent.	Specific Gravity.	Change in Length, per cent.	Change in Weight, per cent.	Specific Gravity.
	Top.	Base.								
After drying 2 months.	+0.126	+0.070	+0.7	+0.108	+0.7	+0.067	+0.3
$\frac{1}{2}$ hour in water in vacuum.....	+0.126	+0.105	+5.3	2.303	+0.079	+1.0	2.298	+0.101	+8.1	2.323
14 hours in water.....	+0.145	+0.122	+5.9	2.301	+0.080	+1.5	2.304	+0.107	+8.4	2.313
38 hours in water.....	+0.160	+0.120	+6.0	2.306	+0.101	+2.0	2.314	+0.110	+8.3	2.320
1 week in water.....	+0.170	+0.135	+6.3	2.313	+0.141	+2.9	2.330	+0.116	+8.4	2.320
1 month in water.....	+0.177	+0.138	+6.6	2.315	+0.146	+3.3	2.341	+0.114	+8.6	2.321
2 months in water....	+0.173	+0.130	+6.8	2.323	+0.146	+3.4	2.342	+0.117	+8.9	2.322
3 $\frac{1}{2}$ months in water...	+0.181	+0.136	+6.9	2.322	+0.143	+3.5	2.348	+0.117	+8.9	2.328
6 months in water....	+0.175	+0.140	+7.0	2.333	+0.150	+3.5	2.351	+0.111	+8.9	2.336

Changes in length and weight are indicated in Fig. 6, and the full data of this last hydration are given in Table III.

It will be noted from Table III that the weights after the 2 months' soaking are without exception slightly greater than those after the previous 2 years' immersion as given in Table II. It will also be seen from Fig. 6 that the bars sawed from the base have attained practically their former maximum length, but that the bars sawed from the rich top coat still lack 0.02 to 0.03 per cent of the length they attained after 2 years' soaking.

Bar No. 156 C apparently had a slight scale chipped off one end just after its immersion so that its first measurement after immersion in water was 0.05 per cent shorter than when dry. If correction were made for this the bar would now be as long as after its previous 2 years' immersion in water.

The figures for change in specific gravity show clearly that a chemical change was taking place in the bars while they lay in water. Their volume increased but their weight increased at a greater rate so that the density rose. The explanation that the free water changed to water of hydration or colloid water is discussed elsewhere. The large amount of gravel in the bar from the base of the walk prevented the figures for specific gravity from showing any material change.

These experiments with bars sawed from a sidewalk taken up after 20 years' service confirm the other experiments in showing that concrete contracts when dry but not to its initial volume, and expands again, when wet for a sufficiently long time, to a volume greater than any which it had previously attained. Concrete exposed to the weather for 20 years had not only failed to become inert but was capable of marked permanent expansion when immersed in water for a long period. This was especially noteworthy in the base, which from its porous structure and position in the ground might have been expected to have reached a condition where further contact with water could not affect it. This is alluded to later in the discussion of concrete sidewalks and pavements.

EFFECT OF HOT WATER ON NEAT CEMENT BARS.

In the spring of 1908 duplicate expansion bars were made from 4 different brands of commercial Portland cement and from 1 brand of Rosendale cement. These bars of neat cement bore the serial number 131, and were made up to compare the effect of boiling with the effect of long storage in cold water. Bars Nos. 131 A, C, E, G and I were kept continuously in water at room temperature for 3 years and were then alternately wet and dried. Their behavior has been discussed earlier in this paper and is shown in Fig. 1. The duplicate bars Nos. 131 B, D, F, H and J were removed from their molds, placed in water for 24 hours and then either boiled or steamed for

24 hours, when they were allowed to cool under water and measured. This treatment was repeated, measurements being made every 2 or 3 days until the total time of boiling or steaming

TABLE IV.—EFFECT OF BOILING AND OF COLD WATER ON BARS OF NEAT CEMENT.

NEAT ROSENDALE CEMENT.

Cement No.	How Treated.	Elongation, per cent.
131 A	Immersed in water at room temperature 3 years.....	0.497
	Alternately wet and dry 2 more years, maximum.....	0.595
131 B	Boiled 24 hours.....	0.545
	Boiled total of 4 days.....	1.210
	Boiled and steamed total of 9 days.....	1.340
	Boiled and steamed total of 15 days.....	1.390
	Boiled and steamed total of 21 days.....	1.434
	Boiled and steamed total of 27 days.....	1.458

NEAT ATLAS CEMENT.

131 C	Immersed in water at room temperature 3 years.....	0.160
	Alternately wet and dry 2 more years, maximum.....	0.168
131 D	Boiled 24 hours.....	0.090
	Boiled 4 days.....	0.115
	Boiled and steamed total of 25 days.....	0.131

NEAT OMEGA CEMENT.

131 E	Immersed in water at room temperature 3 years.....	0.073
	Alternately wet and dry 3 more years, maximum.....	0.110
131 F	Boiled 24 hours.....	0.057
	Boiled and steamed total of 4 days.....	0.077
	Boiled and steamed total of 23 days.....	0.085

NEAT UNIVERSAL CEMENT.

131 G	Immersed in water at room temperature 3 years.....	0.100
	Alternately wet and dry 3 more years.....	0.100
131 H	Steamed total of 3 days.....	0.090
	Steamed total of 7 days.....	0.075
	Steamed total of 22 days.....	0.092

NEAT PENINSULAR CEMENT.

131 I	Immersed in water at room temperature 3 years.....	0.100
	Alternately wet and dry 3 more years.....	0.168
131 J	Steamed 2 days.....	0.075
	Steamed 7 days.....	0.100
	Steamed 18 days.....	0.126

amounted to from 18 to 26 days. The measurements form a consistent series whose course is indicated in Table IV, showing both the effect of cold water and of boiling water.

The boiling test causes greater expansion of the Rosendale cement than the prolonged immersion in water at room temperature. Moreover, the expansion of the Rosendale cement continues on prolonged boiling although at a decreasing rate, until it reaches a total of nearly 1.5 per cent. The four Portland cements give results in close agreement with each other. The expansion on boiling 24 hours is less than that resulting from 3 years' immersion in water at room temperature. Longer boiling or steaming increases the length very slightly and even after 20 days' steaming the length is always less than that of the companion bar which was kept at room temperature in water for 3 years, and then alternately wet and dry for 3 more years.

TABLE V.—EXPANSION OF OLD CEMENT BARS AFTER BOILING.
EXPRESSED AS PERCENTAGE INCREASE IN LENGTH OVER THAT SHOWN WHEN SOAKED IN COLD WATER.

How Treated.	Bar No. 131 C.	Compound Bar, No. 146 VII.	
	Neat Cement.	Neat Portion.	1:3 Sand Mortar Portion.
After boiling 4 hours.....	0.009	0.011	0.003
After boiling 24 hours.....	0.012	0.001	0.008
After boiling 24 hours more.....	0.016	0.006	0.010
Total after boiling 52 hours.....	0.037	0.018	0.021

A few boiling tests have been made on older bars. One of this same series, No. 131 C, when more than 5 years old was taken from water in which it had been immersed 21 days, and boiled for three successive periods with a total time of 52 hours, measurements being made after the bar had cooled under water. The compound bar, No. 146 VII, whose early history has also been given, was subjected to similar treatment when $3\frac{1}{2}$ years old. Table V gives the results expressed as the expansion over the previous length when thoroughly soaked with water. Further tests on these bars are described with the experiments on baking and boiling.

The expansions shown in Table V are small and in general confirm the results of boiling tests on freshly made bars. It seems clear that in hot water as in cold the hydration of the

cement proceeds at a decreasing rate until the colloid is so dense that no more water can penetrate. There can then be no further expansion unless some change, such as a shrinkage due to drying, allows more water to come into contact with cement still unhydrated. The larger expansion of Rosendale cement on boiling is very possibly due to magnesia whose effects, as has been previously shown,¹ manifest themselves very slowly.

EFFECT OF ALTERNATELY BAKING AND BOILING NEAT CEMENT.

The study of progressive changes caused by alternate expansion and contraction at ordinary temperatures proceeds very slowly, and therefore accelerated tests were tried by alternately boiling and baking the bars. These tests at higher temperatures also gave opportunity to test the temperature at which the colloid ceased to be reversible. Studies are here presented of the behavior of a bar of neat cement and one of 1:3 sand mortar when alternately boiled and baked to various temperatures as high as 600° C.

The bar of neat cement chosen was No. 131 C, whose previous changes of length, as it was alternated between air and water for over 5 years, have been shown in Fig. 1. Changes during the boiling and baking tests are shown in Fig. 7. It should be noted that it has been necessary on account of the magnitude of the changes to make the scale of percentage changes in Fig. 7 only one-fourth that of the other figures in this paper. Each series of points on Fig. 7 bears its own number so that the corresponding treatment can readily be determined from the key.

In the first step of the test the air-dried bar was baked for 2 hours at 100 to 105° C., which did not shrink it much. It was then immersed in cold water (2) for 21 days, which brought it back to its accustomed length when wet. Boiling for two periods of 24 hours each (3) caused it to expand only about 0.015 per cent in length each time. The next step was a prolonged drying at 85° C. (4). In the first 24 hours it shrank 0.098 per cent, and the next 0.063 per cent, in the next 3 days 0.028 per cent, and in the following 8 days 0.030 per cent, a total of 0.219 per

¹ Campbell and White, *Jour. of Am. Chem. Soc.*, Vol. 28, p. 1273 (1906).

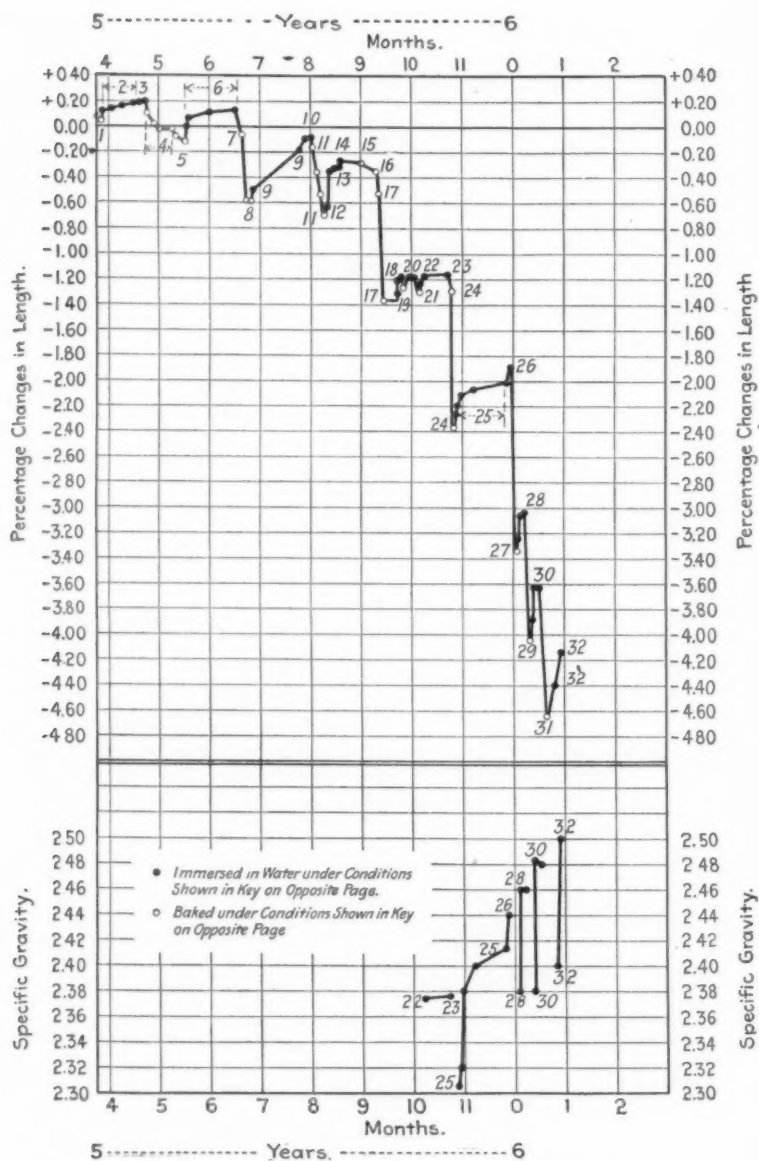


FIG. 7.—Changes in Length and Specific Gravity of a Bar of Neat Cement when Alternately Boiled and Baked.

KEY TO FIG. 7 AND TABULAR SUMMARY OF CHANGES IN LENGTH AND DENSITY OF A BAR OF NEAT CEMENT WHEN ALTERNATELY BOILED AND BAKED.

ALL MEASUREMENTS MADE AT ROOM TEMPERATURE.

Point No.	Description of Test.	Percentage Change in Length Referred to Initial Measurements of Freshly Made Bar.	Specific Gravity	
			Begin-ning.	End.
	Bar air-dried and 5 years 3 months old at beginning of test ...	+0.091
1	Heated 2 hours at 100 to 105° C.....	+0.062
2	In cold water up to 21 days.....	+0.171
3	Boiled for 3 periods with total of 52 hours.....	+0.208
4	Baked at 85° C. for total of 13 days.....	-0.011
5	Baked at 130 to 150° C. for 4 periods of 8 hours each.....	-0.118
6	In cold water up to 30 days.....	+0.139
7	Baked at 90° C. for 24 hours.....	-0.053
8	Baked at 250 to 300° C. for 2 periods of 20 hours each.....	-0.590
9	In cold water up to 30 days.....	-0.190
10	Boiled 2 periods of 24 hours each.....	-0.091
11	Baked for 24 hours each at 85, 150, 250, 340° C.....	-0.708
12	In cold water in vacuum 5 hours.....	-0.673
13	Boiled 3 periods of 24 hours each.....	-0.307
14	Boiled in water under 75 lb. pressure 8 hours.....	-0.287
15	In cold water 2 weeks.....	-0.287
16	Dried in air 1 week and at 85° C. for 48 hours.....	-0.342
17	Baked at 110° C. for 24 hours and at 600° C. for 18 hours.....	-1.358
18	In cold water in vacuum 1 hour and boiled 2 periods of 24 hours each.....	-1.188
19	Baked at 85° C. for 12 hours.....	-1.260
20	Boiled 2 periods of 3 days each.....	-1.162
21	Baked at 101° C. for 24 hours.....	-1.301
22	In cold water in vacuum 1 hour and boiled 36 hours.....	-1.165	2.375
23	In cold water for 23 days.....	-1.157	2.377
24	Baked 24 hours at 101° C. and 24 hours at 475° C.....	-2.388
25	In cold water in vacuum for 1 hour and then in cold water 1 month.....	-2.015	2.305	2.412
26	Boiled 48 hours.....	-1.888	2.441
27	Baked 24 hours at 85° C. and 16 hours at 500° C.....	-3.343
28	In cold water in vacuum $\frac{1}{2}$ hour and boiled for 2 periods of 18 and 36 hours.....	-3.037	2.381	2.460
29	Dried 24 hours at 85° C. and baked 18 hours at 550° C.....	-4.074
30	In cold water in vacuum $\frac{1}{2}$ hour and boiled 2 periods of 36 hours each.....	-3.641	2.379	2.481
31	Dried 24 hours at 85° C. and baked to 600° C. for 18 hours.....	-4.613
32	In cold water in vacuum $\frac{1}{2}$ hour and boiled 36 hours.....	-4.166	2.401	2.501

cent, which brought it very slightly below the zero line and to almost the identical length which it had previously shown when dried in a desiccator for almost 4 months. The temperature of the oven was then (5) raised to 130 to 150° C., at which temperature the bar was baked for four periods of 8 hours each. Most of the change took place in the first 8 hours and the bar shrank to -0.118 per cent, a lower figure than it had ever shown. In cold water (6) it started to expand promptly, and in 30 days expanded a total of 0.25 per cent, which was 50 per cent greater than its normal expansion from the dry state at room temperature to the wet state, so that its final measurement was $+0.139$ per cent. It therefore recovered from its prolonged treatment to 150° C., and after 30 days in cold water expanded to almost the length which it normally showed before it had ever been treated. The bar was then (7) dried at 90° C. for 24 hours and then (8) baked at 250 to 300° C. for 24 hours, which caused a decided contraction to -0.590 per cent. A second heating failed to change this figure. Immersion in cold water for 30 days (9) caused an expansion of 0.400 per cent, which was more than twice that expected of a normal bar, and boiling (10) caused a further expansion of 0.099 per cent. The bar, however, did not return to its former length and remained at -0.091 per cent. A similar cycle (11) with a maximum temperature of 340° C. carried the contraction to -0.708 per cent, which changed after boiling to -0.307 per cent. Boiling under 75-lb. pressure (14) produced little effect except to disintegrate one of the glass plates in the end of the bar so that it had to be removed. From experiments on another bar it was judged that this boiling under pressure caused an expansion of 0.02 per cent.

Further cycles of baking and boiling were carried out with similar changes occurring. The maximum furnace temperature was 600° C., and the final result after nine bakings at temperatures of 100° C. and above, was a length of -4.613 per cent.

After each baking, the bar was immersed in cold water and in all the later tests the air was exhausted from the vessel during the first immersion so that the occluded air was pumped out. An expansion of as much as 0.200 per cent sometimes took place in the first hour, but a greater expansion took place later. Measurements of specific gravity were made in the later tests in an

attempt to follow the mechanism of expansion. These changes in specific gravity are shown graphically in Fig. 7 and given numerically in the key to Fig. 7. It will be seen that there are large increases in the density. This is especially well shown in (25), where after baking to 475° C. and keeping in water in vacuum for $\frac{1}{2}$ hour, the specific gravity was 2.305. It rose after 30 days in cold water as is shown graphically in Fig. 7 to 2.41, and on boiling to 2.441. With each successive baking the length of the bar decreases and the density increases. The specific gravities taken after $\frac{1}{2}$ hour in water in vacuum do not change so regularly with the successive heatings, but those after thorough reaction with water show marked and very progressive increases, rising from 2.377 in (23) to 2.441 in (26), to 2.460 in (28), to 2.48 in (30), and to 2.501 in (32), which is the end of the series.

The evidence here presented shows that alternate expansion and contraction is shown in even greater measure by cements baked up to 600° C. when immersed in water, than is shown by normal cements. It also shows that the colloid of cement does not become irreversible up to a dull-red heat. The only other possible explanation for the slow expansion after the capillaries had become filled with water is the assumption that the colloid had indeed been permanently dehydrated, but that new portions of unchanged clinker were now coming in contact with water and thus reacting with it for the first time. This assumption is not tenable in view of the figures for specific gravity. The specific gravity of neat Portland cement is about 3.10. The hydrated cement even after calcination at red heat must be less than this. If, then, clinker was constantly being converted into material of lower density the density of the whole mass would have to decrease instead of increasing as it does.

The only feasible explanation for these changes in length and density is that the colloid of Portland cement remains capable of reaction with water even after heating to 600° C. The bar shrinks with the dehydration, and when again placed in water there is less interstitial space for the water to occupy and consequently not all of the colloid can become hydrated again and the bar remains permanently shrunken. The specific gravity, however, is higher, for the bar consists of the same amount of unchanged clinker, a larger portion of dehydrated

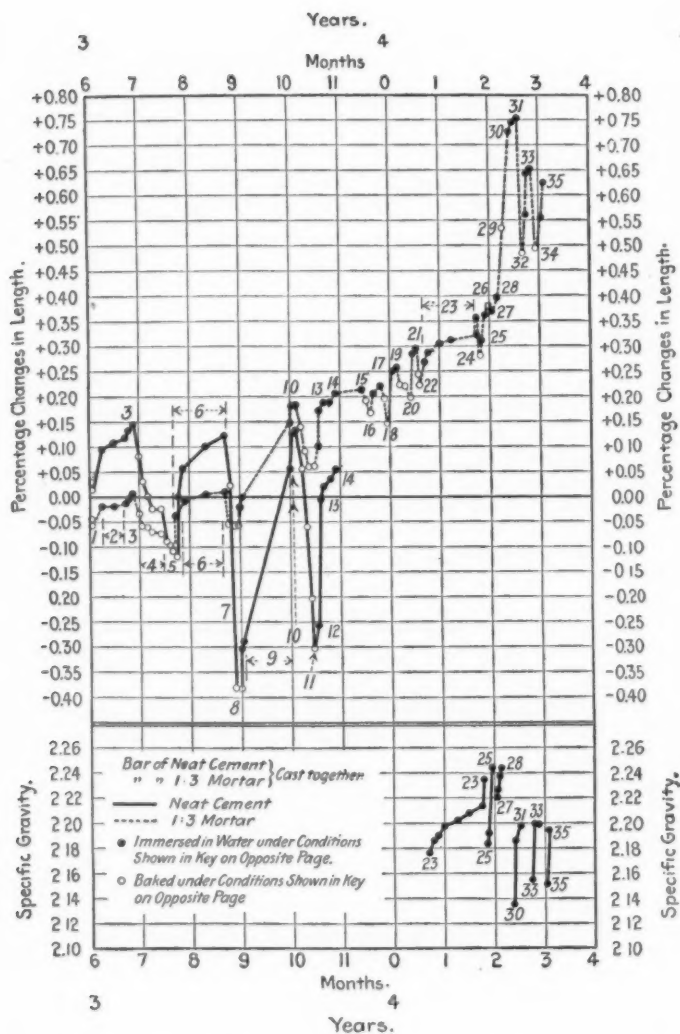


FIG. 8.—Changes in Length of a Compound Bar when Alternately Boiled and Baked.

KEY TO FIG. 8 AND TABULAR SUMMARY OF CHANGES IN LENGTH AND DENSITY OF A COMPOUND BAR OF NEAT CEMENT AND 1:3 SAND MORTAR WHEN ALTERNATELY BOILED AND BAKED.

ALL MEASUREMENTS MADE AT ROOM TEMPERATURE.

Point No.	Description of Test.	Percentage Change in Length Referred to Initial Measurement of Freshly Made Bar.		Specific Gravity.	
		1:3 Mortar.	Neat.	Beginning.	End.
	Bar air-dried and 3 years 6 months old at beginning of test	-0.048	+0.027
1	Baked 2 hours at 100 to 105° C.....	-0.062	+0.013
2	In cold water up to 21 days.....	-0.014	+0.120
3	Boiled 3 periods with total of 52 hours.....	+0.007	+0.138
4	Baked at 85° C. for total of 13 days.....	-0.074	-0.025
5	Baked at 130 to 150° C. for 5 periods of 8 hours each.....	-0.084	-0.116
6	In cold water up to 30 days.....	+0.011	+0.124
7	Baked at 90° C. for 24 hours.....	-0.052	-0.022
8	Baked at 250 to 300° C. for 2 periods of 24 hours each..... (At the end of this treatment the bar of neat cement was cracked completely through near the middle and showed other hair cracks.)	-0.059	-0.373
9	In cold water up to 30 days.....	+0.145	+0.055
10	Boiled for 2 periods of 24 hours each.....	+0.180	+0.131
11	Baked for 24 hours each at 85, 150, 250 and 340° C.....	+0.063	-0.303
12	In cold water in vacuum 5 hours.....	+0.098	-0.255
13	Boiled 3 periods of 24 hours each.....	+0.187	+0.035
14	Boiled 8 hours in water under 75 lb. pressure..... (The neat portion which had cracked through in several places was sawed off and discarded, leaving the 1:3 sand portion intact.)	+0.208	+0.054
15	In cold water for 2 weeks.....	+0.208
16	Baked for 24 hours each at 85 and 110° C.....	+0.160
17	In cold water in vacuum 24 hours, then boiled 24 hours.....	+0.220
18	Baked 24 hours at 85° C. and 24 hours at 450° C.....	+0.140
19	Boiled 2 periods of 24 hours each.....	+0.253
20	Baked for 24 hours each at 85, 100 and 450° C.....	+0.199
21	Boiled for 2 periods of 24 hours each.....	+0.297
22	Baked 24 hours at 100° C. and 12 hours at 450° C.....	+0.220
23	In cold water up to 43 days and then boiled 24 hours.....	+0.358	2.177	2.236
24	Baked at 400° C. for 18 hours.....	+0.277
25	In cold water 7 hours and boiled 18 hours.....	+0.363	2.182	2.245
26	Frozen while wet, at +29° F. and measured while frozen.....	+0.381
27	After thawing out under water.....	+0.368	2.226
28	Boiled for 48 hours.....	+0.390	2.238
29	Heated rather rapidly to 500° C. and held there 18 hours.....	+0.529
30	In cold water in vacuum $\frac{1}{2}$ hour.....	+0.725	2.145
31	Boiled 2 periods of 24 hours each.....	+0.752	2.199
32	Baked at 550° C. for 18 hours.....	+0.482
33	In cold water in vacuum $\frac{1}{2}$ hour, then boiled for 2 periods of 36 hours each.....	+0.655	2.155	2.198
34	Baked to 600° C. for 18 hours.....	+0.482
35	In cold water in vacuum $\frac{1}{2}$ hour, then boiled 36 hours.....	+0.625	2.151	2.195

colloid, and a smaller portion of hydrated colloid than it had before its last baking.

EFFECT OF ALTERNATELY BAKING AND BOILING A COMPOUND BAR.

Two compound bars, Nos. 146 II and 146 VII, of the series previously described, were subjected to the same boiling and baking tests as the neat bar No. 131 C. The results are, however, so strikingly different as to require separate description. It will be recalled that these compound bars were made by casting a bar of neat cement 1 in. square and 4 in. long on top of a freshly made bar of 1:3 sand mortar. The changes in length of four of these bars as alternately wet and dried at room temperature are shown in Fig. 5. At the end of $3\frac{1}{2}$ years two of the bars were tested at higher temperatures. The results for bar No. 146 VII are shown in Fig. 8 and the accompanying key and tabular summary.

It would have been expected that both portions of the bar would shrink when baked and expand when dry, but at an unequal rate. The query was as to the magnitude of the differential stresses and which portion of the bar would first show distress. The curves of Fig. 8 show that the neat portion suffered greater shrinkage than the sand portion, which is not unexpected. The unexpected part was that the sand portion refused to shrink materially and that therefore the neat portion cracked completely in two. After heating (8) to 300°C . the neat bar had shrunk 0.39 per cent while the sand portion had contracted only 0.01 per cent. After thorough expansion in water and again baking for several periods (11) to a maximum of 340°C ., the sand portion of the bar instead of shrinking had actually expanded to a reading of +0.063 per cent, which represented an increase of +0.122 per cent over that which it had attained after its previous baking at a lower temperature. The neat portion, being cracked, was affected by the expansion of the sand portion and did not shrink as much as it had before. The neat portion, being useless, was sawed off and the tests continued with the mortar portion of the bar alone. Successive alternations of baking and boiling caused the bar to progressively

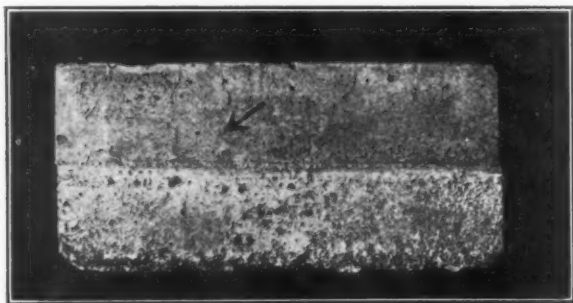
expand as is shown in Fig. 8. We have therefore the curious effect of the same series of tests causing a shrinkage of 4.6 per cent in a bar of neat cement and an expansion of 0.75 per cent in a bar of 1:3 sand mortar.

The same explanation holds good here, however, as held for the progressive expansion of bars when alternately wet and dried at room temperature. The expansive power of the hydrating cement is sufficient to elongate the bar. The grains of sand lock in their new position and the contractive force of the colloid is not sufficient to bring them back to their old positions. The bar is therefore more porous than before and when immersed in water, a larger amount of water will enter and new clinker will be hydrated with further expansion. The specific gravity curves show similar increments, while the bar reacts with water, that the bar of neat cement showed, so the reaction is entirely analogous.

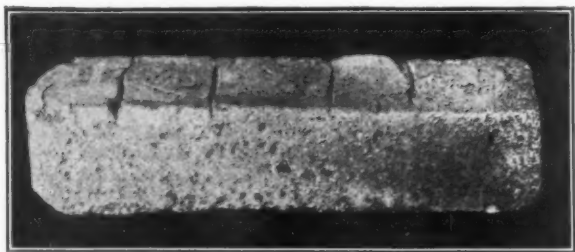
There is one exception to the usual effect of heating shown at point (29). The bar after boiling in water had shown a length (28) of $+0.390$ per cent. It was then dried over night at about 85°C . as was customary and placed in the electric muffle furnace. In the ordinary procedure the muffle furnace was cold when the test was started, and so the bar and the furnace heated slowly and gradually together. On this occasion the furnace was hot, although not at its full temperature of 500°C . When the bar was removed after 18 hours' heating it was found to have expanded instead of contracted. The specific gravity when placed in water was also markedly lower. The most probable explanation of this anomalous behavior is that steam was generated rapidly enough within the bar to cause a pressure within the bar, which expanded it. A bar expanded in this way should have the lower density which actually appeared. Subsequent bakings and boilings caused alternate expansion and contraction as before, but the high point reached after the rapid heating has not yet been attained again.

The appearance of this bar at the conclusion of these tests is shown in Fig. 9 (b). When the neat portion of the bar was sawed off a thin skin of the neat cement was left adhering to the sand portion. The cracks in the neat portion barely visible in Fig. 9 (a) have opened up during the eight baking and boil-

ing treatments given after the neat portion was sawed off until now they appear as gaping cracks into which the thumb nail may easily be inserted. The sand portion has expanded as shown by the curves and the neat portion left adherent has undoubtedly contracted, giving an additive width to the cracks.



(a) Appearance after Heating to 340°C . (Point No. 11).
The arrow indicates the crack which has broken the bar into two entirely separate pieces.



(b) Appearance of Sand Portion of Same Bar at End of Tests.

FIG. 9.—Effect of Alternate Boiling and Baking on the Compound Bar of Fig. 8.

The history of this compound bar, No. 146 VII, has been given in detail. A similar test was made on bar No. 146 II with entirely similar results up to the point when the bars were to be sawed apart. Unfortunately the sand portion of the bar broke during this process as well as the neat portion, and the test could not be continued.

EFFECT OF FREEZING CEMENT BARS WHILE WET.

Water expands about 8.5 per cent while freezing, an amount so large that its effect could be readily measured if even a relatively small amount of liquid water was present in the bar when frozen. Tests of three bars of neat cement of different brands, and three bars of mortar made from one part of cement and three of Ottawa sand are here presented. All of these bars were at least 3, and some 6 years old. They had been kept alternately wet and dry, and at practically constant temperature all the time, and had been in water continuously for 2 months just prior to the tests. The full histories of all but one are shown graphically in Figs. 1 and 4.

In making these tests the bars were measured in the constant-temperature room, and then both micrometer and bars were placed outdoors in a light box to protect them from snow. The micrometer frame was of cast iron and the accuracy of its measurements was checked regularly against a companion bar of the same lot of iron and a companion bar made from invar steel. The measurements of the cast-iron companion bar were almost identical at all temperatures, the greatest variation from the mean in ten successive series being 0.002 per cent, which is within the error of reading the instrument. The direct measurement on the bars when frozen gave the differential change in length compared with cast iron.

Table VI shows the relative change of length as compared with cast iron, and also the changes in specific gravity of the bars after their successive freezings.

None of the neat-cement bars show any expansion on freezing. On the contrary they contract at a rate which is nearly identical with that of cast iron. Two of the bars show a slightly greater contraction than cast iron, but the maximum total difference for a temperature drop of from $+70$ to $+5^{\circ}$ F. is only 0.01 per cent—an amount which is practically negligible. After four successive freezings and thawings, the bars were entirely unaffected in length and specific gravity.

Two of the three sand-mortar bars agree with the neat bars in showing practically the same contraction as cast iron. The third bar, No. 150 G, shows not only a relative but an absolute expansion when frozen. The magnitude of the expansion

TABLE VI.—CHANGES IN LENGTH AND SPECIFIC GRAVITY OF CEMENT BARS
FROZEN WHILE WET.

BAR NO. 131 E, NEAT CEMENT, MADE MARCH 11, 1908.

Date (1914).	Temperature of Freezing, deg. Fahr.	Length Expressed in Percentage Change from Initial Measurement of Freshly Made Bar.		Change Com- pared with Cast Iron. ¹	Specific Gravity.
		In Water at Room Temp.	Frozen.		
Feb. 21.....	+0.761	2.217
" 23.....	+5	0.730	-0.011
" 23.....	0.760	2.218
" 24.....	+6	0.751	-0.009
" 24.....	0.763	2.219
" 25.....	+9	0.753	-0.010
" 25.....	0.760	2.217
" 26.....	+23	0.755	-0.005
" 26.....	0.761	2.217
Average.....	0.761

BAR NO. 131 G, NEAT CEMENT, MADE MARCH 13, 1908.

Feb. 21.....	+0.823	2.216
" 23.....	+5	+0.809	-0.014
" 23.....	0.828	2.216
" 24.....	+6	0.820	-0.008
" 24.....	0.826	2.217
" 25.....	+9	0.817	-0.009
" 25.....	0.829	2.217
" 26.....	+23	0.826	-0.003
" 26.....	0.829	2.212
Average.....	0.827

BAR NO. 150 B, NEAT CEMENT, MADE NOVEMBER 4, 1910.

Feb. 21.....	+0.718	2.228
" 23.....	+5	+0.722	+0.004
" 23.....	0.718	2.228
" 24.....	+6	0.716	-0.002
" 24.....	0.718	2.226
" 25.....	+9	0.716	-0.002
" 25.....	0.718	2.226
" 26.....	+23	0.719	+0.001
" 26.....	0.720	2.224
Average.....	0.718

BAR NO. 150 G, CEMENT AND OTTAWA SAND (1:3), MADE NOVEMBER 22, 1910.

Feb. 21.....	+0.720	2.221
" 23.....	+5	+0.836	+0.116
" 23.....	0.785	2.207
" 24.....	+6	0.890	+0.115
" 24.....	0.815	2.202
" 25.....	+9	0.909	+0.094
" 25.....	0.829	2.198
" 27.....	+31	0.835	+0.006
" 27.....	0.832	2.203
Mar. 3.....	+18	0.897	+0.065
" 3.....	0.850	2.197

¹ The change in length is expressed as the differential change compared with cast iron. That is, if the cement shrinks at exactly the same rate as cast iron, the reading for all temperatures will be 0.000.

TABLE VI (Continued).—CHANGES IN LENGTH AND SPECIFIC GRAVITY OF CEMENT BARS FROZEN WHILE WET.

BAR No. 146 C₁, CEMENT AND OTTAWA SAND (1:3), MADE NOVEMBER 30, 1909.

Date (1914).	Temperature of Freezing, deg. Fahr.	Length Expressed in Percentage Change from Initial Measurement of Freshly Made Bar.		Change Com- pared with Cast Iron. ¹	Specific Gravity.
		In Water at Room Temp.	Frozen.		
Feb. 21.....	+0.722	2.366
" 23.....	+5	0.724	+0.002
" 23.....	0.723	2.362
" 24.....	+6	0.735	+0.012
" 24.....	0.723	2.363
" 25.....	+9	0.730	+0.007
" 25.....	0.721	2.362
" 26.....	+23	0.726	+0.005
" 26.....	0.723	2.363
Average.....	0.722

BAR No. 146 C₂, CEMENT AND OTTAWA SAND (1:3), MADE NOVEMBER 30, 1909.

Feb. 24.....	+0.754	2.374
" 25.....	+9	0.775	+0.021
" 25.....	0.755	2.370
" 26.....	+23	0.772	+0.017
" 26.....	0.755	2.375
" 27.....	+31	0.760	+0.005
" 27.....	0.758	2.367
Mar. 3.....	+18	0.764	+0.006
" 3.....	0.757	2.372
Average.....	0.756

¹ The change in length is expressed as the differential change compared with cast iron. That is, if the cement shrinks at exactly the same rate as cast iron, the reading for all temperatures will be 0.000.

is considerable, being nearly 0.10 per cent in each of four out of five freezings. On the fifth occasion the temperature was only +31° F., and it is very doubtful if the bar became thoroughly frozen. The behavior of this bar cannot be laid to the cement, for the neat bar, No. 150 B, made from the same cement behaved normally.

The explanation of the abnormal behavior of this bar, No 150 G, may be found by a study of its water absorption after having been 3 months in damp air. Figures are available for all of the bars which were frozen and are given in Table VII.

It will be noted that none of the bars of neat cement gained more than 2 g. in weight when after saturation with damp air they were immersed in water in a vacuum. One of the mortar bars, No. 146 C₁, showed an equally slight increase in weight, another, No. 146 C₂, gained 8 g., while the third, No. 150 G,

gained 11 g. This increase in weight on immersion in water may be properly considered as liquid water in the capillaries of the bar. It is the bar, No. 150 G, with the large amount of capillary water which shows the large expansion on freezing. This expansion is shown not only while the bar is frozen, but is partially maintained after the bar thaws, so that after five freezings the bar has suffered a permanent elongation of 0.130 per cent.

This change is reflected in the specific gravity of the bar as shown in Table VI. The specific gravity of this bar, No. 150 G, drops from 2.221 to 2.207 after its first freezing and thawing. It is the only bar of the series to show material change in this respect. The surprising fact about this series is not that this

TABLE VII.—WEIGHTS OF BARS AFTER SATURATION AND IMMERSION.

BAR No.	Weight after Saturation in Damp Air, g.	Weight after Subsequent Immersion in Water in Vacuum, g.	Weight of Water Absorbed in Capillaries, g.
131 E.....	137.0	137.6	0.6
131 G.....	142.3	144.0	1.7
150 B.....	142.9	144.8	1.9
150 G.....	130.1	141.5	11.4
146 C ₁	149.8	151.5	1.7
146 C ₂	141.8	149.9	8.1

bar, No. 150 G, expands on freezing but that the bar No. 131 C₂, which absorbs two-thirds as much capillary water, shows so little expansion on freezing. It is possible by a careful study of the life history of these bars to give some explanation of the variations which they show in this respect. The length which this paper has already reached makes it necessary, however, to postpone it to another occasion.

SUMMARY OF RESULTS.

The tests here reported confirm the theory that Portland cement in reacting with water forms a colloid which expands when wet and shrinks when dry. It does not become irreversible and lose this property even after 20 years' exposure to the weather or after heating to 600° C. The length of a bar which

has passed through a cycle of drying and wetting is not usually the same as it was before the cycle, but may be longer or shorter. Under ordinary conditions the bar remains elongated because water acts more rapidly, and the compressive strength of the expanding colloid is more than the tensile strength of the contracting colloid. This is especially true in concrete or in mortar where the particles of aggregate forced apart by the expansion lock in their new positions so that the bar remains permanently elongated. With neat cement, where the only aggregates are the unattacked particles of clinker, the bar may be made to grow shorter. By suitable alternations at elevated temperatures a bar of neat cement has been made to shrink in length 4.6 per cent and under identical conditions, a bar of 1 : 3 sand mortar to elongate 0.75 per cent. Bars sawed from a sidewalk taken up in good condition after 20 years' service were made to attain a further elongation of 0.175 per cent for the top coat and 0.130 per cent for the base after a series of prolonged wettings and dryings at room temperature. This is an expansion equivalent to that which might be expected from a temperature variation of 300° F.

Concrete which has been properly made, and in which the colloid has been properly developed, has its interstitial spaces so filled with dense colloid when it is thoroughly soaked in water that there is no liquid water present in the pores. Such concrete may be frozen without any expansion. In fact it will contract with temperature change at almost exactly the same rate as cast iron. If the concrete has been poorly prepared or if there has been insufficient opportunity for the cement to develop its colloid by reacting with water, then its pores when wet will contain liquid water which will expand as usual on freezing, with possible resultant disintegration.

PRACTICAL APPLICATIONS.

Concrete kept constantly wet from the time it is poured will expand rather slightly and after a few years will be practically in equilibrium with its surroundings, and will not suffer further change since the pores have become filled with a dense colloid, which prevents water from acting on the unchanged cement. Such concrete will contract at low temperatures at the same rate

as cast iron, but will otherwise be unaffected by freezing, and may be considered a permanent structure. It will be in compression throughout because of the development of the colloid and will therefore be strong. The slow development of the colloid of the cement in contact with water explains the decrease in leakage frequently shown by dams and reservoirs after they have been put into service.

Concrete which is allowed to dry soon after it is poured, and which thereafter remains exposed to the air but protected from rain, will be weak because its colloid has been insufficiently developed. Further, it will—if its ends are restrained—be in tension because it shrinks as the colloid becomes dehydrated. Even this concrete will, however, react with the moisture of the air and will probably gain strength in the process, so that concrete used in the interior construction of buildings will probably meet its severest test within the first few months after it is poured. Concrete of this type will nevertheless have a relatively poorly developed colloid and will be porous. If it becomes wet and freezes, it will expand and the expansion may be permanent.

Concrete which is exposed to the weather is put to a very severe test. It expands and contracts when alternately wet and dried and probably retains this property indefinitely. It has no stable condition but is always slowly changing. The changes are progressive, especially where the concrete is occasionally kept thoroughly wet for perhaps a month at a time. This period is sufficient to cause the colloid to expand quite fully. At first the only effect is to make the concrete more dense and to fill up the pores. After several years the mass begins to expand as a whole. The particles of gravel are forced apart and, wedging in their new positions, fail to return when the concrete dries again. The next time the concrete is wet an added quantity of water enters and reacts with still unchanged cement with the result that there is larger expansion. This progressive elongation is the cause of the pressure ridges and upheavals found frequently in old cement sidewalks. It has been shown in this paper that sidewalks after 20 years' service still possess this capacity for further progressive expansion. This phenomenon is of course more serious in damp climates than in dry ones, and in moist locations than those which are well drained. The author has seen cement sidewalks in exceptionally well-drained

situations which showed very little signs of deterioration after 20 years' of service.

When concrete dries the surface dries relatively rapidly and the shrinkage is considerable, especially if it is rich in cement. This shrinkage of the surface while the rest of the structure is still wet and expanded, causes hair cracks to appear on the surface even when the unit is small and entirely unrestrained, as was the case in bar No. 146 A, IV, of Fig. 3. Each subsequent expansion and contraction will cause this hair crack to deepen. The disintegrating effect will undoubtedly be accentuated by impacts such as concrete highways receive from traffic.

The changes of volume due to moisture are more apparent with mixtures rich in cement than with lean mixtures. In sidewalks and pavements it would therefore be expected that the rich top coat would expand and contract to the greater extent. This is frequently the case and its results may be seen in sidewalks where the top has split off the base, due to differential expansion. However, the rich mixture with its greater tensile strength tends to contract to its original length more perfectly than the mixture with less cement and more gravel, and in pavements and sidewalks the leaner base is buried in the ground where it remains wet for a longer period. Under proper conditions, therefore, the base may ultimately expand more than the top coat and give the concave effect to a walk or pavement which is not infrequently observed.

Frost is not much to be feared as an initial instrument of disintegration of properly mixed concrete which has been given a fair chance to develop its colloid, since the amount of liquid water in the pores of the concrete is too small to cause serious expansion. If disintegration has started from other causes, water may enter in larger quantities, and on freezing may cause serious injury.

If concrete, and particularly rich concrete, is to remain exposed to the weather, the only way to keep it in good condition is to so place it or protect it so that it will not be subject to great fluctuations in its moisture content. The most trying conditions are those which concrete highways have to meet. As pavements are now being laid, they seem certain in many localities to prove a serious disappointment to those who are building them with the expectation that they will be permanent structures.

DISCUSSION.

The Chairman. **THE CHAIRMAN (PRESIDENT ARTHUR N. TALBOT).**—Concrete evidently has the property of India rubber. If there is any written discussion on this paper, the Secretary will read it. There being none, the paper is open for discussion.

Mr. Wilson. **MR. P. H. WILSON.**—In the last paragraph of Mr. White's paper, the conclusion is drawn that concrete pavements laid where exposed to fluctuations of moisture content will not prove permanent structures.

Up to the close of 1913 there were built in the United States over 18,500,000 sq. yd. of concrete pavements, about 1,000,000 sq. yd. of which were built prior to 1909. These concrete pavements have been built in 44 different states under every variety of conditions imaginable. Practically every pavement from Utah east has been examined by some of our men, and we have yet to receive a report of a pavement which has failed due to the expansion of the concrete caused by fluctuations in moisture content.

Poor drainage, improper sub-base, dirty sand and stone, porous mixtures, careless proportioning of the materials, and lack of care in protecting the concrete during the curing period have all caused partial failures, and if a concrete road is to be successful, all must be given attention.

I therefore believe, in consideration of the many successful concrete pavements which have been built and are in service to-day, that the conclusion reached in the paper as to their permanence is unwarranted.

Mr. Johnson. **MR. N. C. JOHNSON.**—May I ask Mr. White if what he ascribes to a sort of "ratchet action" on the part of the sand grains, whereby expansion is permitted but contraction prevented, may not be more rationally explained on the ground of minute voids and disruptive physical actions taking place within them, due to the extremes of heat and cold to which the concrete was subjected? It seems hardly permissible to impose so unusual a function on chemically inert and physically unchanging grains of sand.

MR. RICHARD L. HUMPHREY.—Mr. Wilson has referred to **Mr. Humphrey.** concrete roads. I should like to call attention to the concrete sidewalks that have been in use in this country since 1882, and which have endured without developing cracks or other objectionable defects. I am sure that if the India-rubber characteristics described by Mr. White had been true of these sidewalks, they would in consequence have moved into the street.

The paper is extremely interesting, and throws much light on the action of moisture on cement mortars and concretes; and it would appear that as the character of the cement changes in regard to the relation of the colloids to the crystalloids,—that is, as the crystalloids increase,—the effect of moisture decreases. After studying Mr. White's paper, I have come to think that due consideration should be given to the fact that in the test piece which he used the surface was very large in proportion to the mass, and hence, this small mass would be more susceptible to the action of moisture than the very large masses in which concrete is used in actual work. I believe, therefore, that Mr. White's conclusions, based on a small mass, cannot possibly apply to a large mass. I agree with Mr. Wilson that the closing paragraph of the paper is not warranted in the light of practical experience with both concrete sidewalks and roadways.

MR. W. K. HATT.—I was interested in reading the paper **Mr. Hatt.** and noting the reference to a sidewalk that had been laid 20 years ago. It was, as I judged from the context, in good condition. It had not moved, although it had been in place 20 years. The author of the paper took up samples of this and submitted them to a laboratory process. It does not seem to me that what the author did to the samples from this sidewalk in the laboratory should have much bearing upon the previous good behavior of the sidewalk.

MR. R. J. WIG.—It seems to me that the author has mis- **Mr. Wig.** interpreted his results. A cement which after having been subjected to the treatment some of the author's specimens have undergone remains intact, would apparently remain intact for a great many years in service. Certainly the author's conclusions, both in the summary where he states very positively that it must be considered a grave source of danger to concrete highways in wet locations, and in the last paragraph, which has been quoted,

Mr. Wig. are unwarranted. The author entirely overlooks the fact that the behavior of restrained concrete may be entirely dissimilar from that of unrestrained concrete.

A Member. A MEMBER.—I think this question of cracks a rather interesting one. I am of the opinion that hair cracks are present in rich mortars from the first; that is, they start at once and develop very quickly. The fact that they are not seen at once is a question of weathering, and of dirt and dust working into the cracks and gradually showing them up. I do not think that cracks are a development with time; but rather that their presence is only gradually revealed.

Mr. Force. MR. H. J. FORCE.—I should like to ask Mr. White whether all the specimens used in the expansion test passed the hot test prescribed in the standard specifications of the Society, and whether or not this sample of cement failed on the hot test?

Mr. White. MR. A. H. WHITE (*Author's closure, by letter*).—Several members hold that the author's fears for the permanence of concrete pavements laid in unfavorable environment are unwarranted. Mr. Wilson bases his opinion on reports of the condition of numerous pavements. But only a small fraction of these pavements were over four years old. The destructive action referred to in the paper is slow and progressive, and before injurious expansion can become evident the internal voids in the concrete must have been filled and the expansion joints closed. The fact that a pavement has lasted four years is no proof that it will last ten or twenty. Mr. Humphrey refers to the sidewalks which have been in use in this country since 1882, but does not make any estimate of the proportion of cement walks which have thus been in service twenty or thirty years and are still in fair condition. It was the conspicuous failure of sidewalks which led the author to undertake some of the tests here reported. Mr. Humphrey also believes that conclusions cannot be drawn for small test pieces. A small test piece will certainly come into equilibrium with water more rapidly than a larger one, but size cannot affect chemical equilibrium. Mr. Hatt holds that laboratory tests on bars from an old sidewalk do not have much bearing upon the previous good behavior of the walk. They show that the previous good behavior was the result of good environment and not of any

property inherent in the cement or the concrete. They also **Mr. White.** show that the sidewalk still possessed the capacity of reacting to an unfavorable environment. Mr. Wig charges the author with overlooking the fact that the behavior of restrained concrete may be entirely dissimilar from that of unrestrained concrete. Mr. Wig probably means that the behavior of the two will be different in degree, which is of course true. Restrained concrete will be subject to the same type of deformation as the unrestrained. In answer to Mr. Force's question: all the Portland cements used in these tests passed the hot tests.

There is little question but that concrete will be permanent in a favorable environment. Its lack of permanence in some cases needs no demonstration. The present paper has shown that certain conditions of environment by no means unusual, cause dangerous changes of volume, and the warning should not be lightly minimized.

ADDITIONAL RESULTS OBTAINED WITH THE AUTO-CLAVE TEST FOR PORTLAND CEMENT.¹

BY H. J. FORCE.

The object of this paper is to give results of additional tests on various brands of Portland cement up to and including one-year tests, of which some failed under and others passed the autoclave test. The method of making these tests has already been described in a previous paper.²

In Fig. 1, lines Nos. 1 to 5 represent the results from the completed tension tests which had been partly shown in detailed tabulated form in the earlier paper above referred to,³ each line representing the tests for the mill of corresponding number.

Line No. 3 shows the results from tests of five samples at the age of one year. The other four samples, referred to in my previous paper, were tested only up to 28 days. The five samples tested up to one year show a tensile strength of from 205 to 597 lb.

It is believed that short-time tests, that is, tests up to and including one year, and especially tests on small briquettes for tensile strength, will not show very great variations, that is where a large number of samples are taken together as was done in this case. We also note a considerable improvement in cements in general with regard to the autoclave test. Most cements at the present time show a tensile strength with the autoclave of from 200 to 300 lb., while two or three years ago the majority of cements showed very little or no tensile strength with the autoclave test, many disintegrating entirely.

Fig. 2 represents the results of the compressive tests of cement from the same mills for which the results of the tensile tests are shown in Fig. 1. Some of the samples used in the

¹ Supplementing the paper on this subject presented at the last annual meeting.

² *Proceedings, Am. Soc. Test. Mats.*, Vol. XIII, p. 740 (1913).

³ *Ibid.*, pp. 743-746.

latter tests were also used in the former. In Fig. 2 the cements from mills Nos. 1, 2 and 4 passed the autoclave test, while none of the cement from mill No. 3 and only a small portion of that from mill No. 5 passed that test. The cement from mill No. 6 entirely disintegrated in the autoclave test, while that from mill No. 3 and mill No. 5 showed a tensile strength of from 150 to 300 and 400 lb. The tests from mill No. 6 represent a shipment of three carloads.

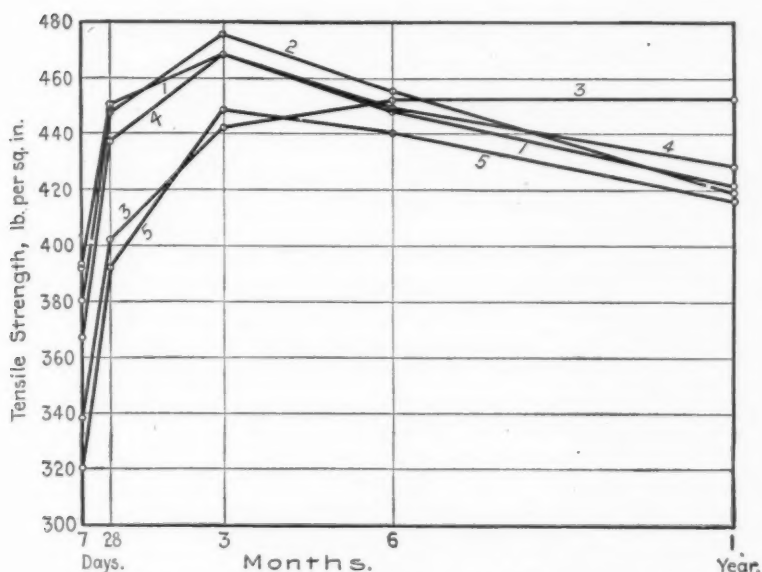


FIG. 1.—Results of Completed Tension Tests of Cements from Mills Nos. 1 to 5; 1 part Cement to 3 parts Sand.

In Table I are shown the number of samples for each test period in Fig. 2. It will be seen that only seven samples from mill No. 3 were tested since only seven shipments had been made from this mill during this period. Each sample represents the average of three carloads of cement.

Table II shows the results of tests on neat briquettes placed in water for the indicated periods. At the end of each period four briquettes were taken from the water, two being broken

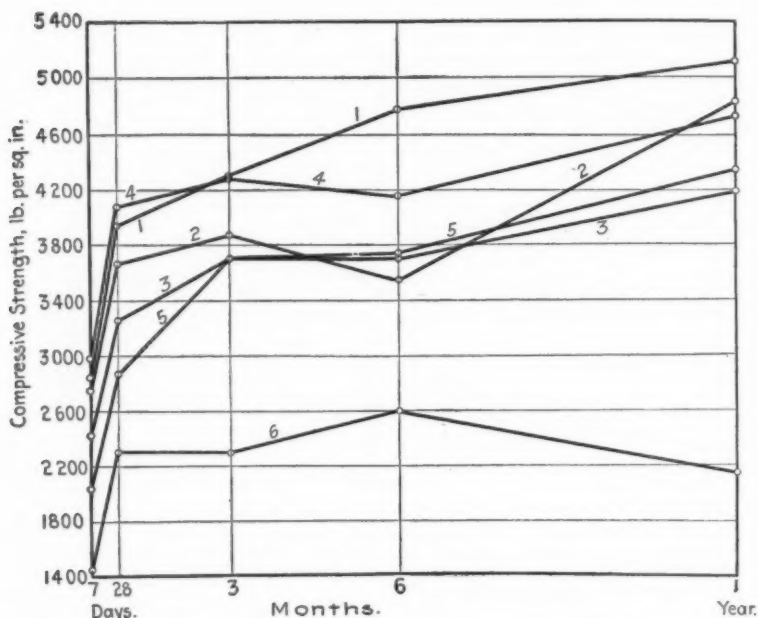


FIG. 2.—Results of Compression Tests of Cement from Mills Nos. 1 to 6; 2-in. Cubes, 1 part Cement to 3 parts Sand.

for tensile strength and two being placed in the autoclave. The two broken for tensile strength were ground up to pass through a 100-mesh sieve, dried at 212° F., and the specific gravity and loss on ignition were then determined. The briquettes passing the autoclave test were treated in the same manner.

TABLE I.—TESTS OF 2-IN. CUBES.

Cement from Mill No.	Number of Samples.					Remarks.
	7 days.	28 days.	3 months.	6 months	12 months	
1	68	68	63	58	19	5-year test made.
2	37	37	32	30	5	5-year test made.
3	7	7	7	6	5	1-year test made.
4	21	21	21	21	7	5-year test made.
5	24	24	22	22	8	5-year test made.

TABLE II.
CEMENT "5-1000"

Period Tested.	Two Briquettes from Water.			Two Briquettes placed in Autoclave.		
	Tensile Strength, lb. per sq. in.	Specific Gravity.	Loss on Ignition, per cent.	Tensile Strength, lb. per sq. in.	Specific Gravity.	Loss on Ignition, per cent.
24 hours.....	305	2.70	11.26	500	2.62	7.00
2 days.....	419	2.40	9.40	470	2.50	7.50
4 days.....	442	2.37	8.85	483	2.48	10.55
8 days.....	565	2.34	11.50	460	2.44	11.90
16 days.....	578	2.26	12.95	525	2.43	11.40
1 month.....	619	2.28	12.80	551	2.45	11.65
2 months.....	613	2.10	13.65	479	2.37	12.50

CEMENT "5-2000."

24 hours.....	304	2.65	7.64	60	2.61	10.44
2 days.....	413	2.40	7.60	55	2.50	10.68
4 days.....	442	2.34	8.75	63	2.48	11.55
8 days.....	516	2.26	12.20	50	2.50	12.15
16 days.....	578	2.21	11.05	55	2.41	13.25
1 month.....	634	2.26	12.85	91	2.36	15.45
2 months.....	615	2.26	14.90	453	2.37	13.60

CEMENT "149."

24 hours.....	340	2.44	8.65	disintegrated	2.30	14.00
2 days.....	615	2.37	11.55	"	2.41	13.55
4 days.....	663	2.278	13.78	"	2.278	14.24
8 days.....	835	2.327	12.55	"	2.396	14.35
16 days.....	877	2.33	14.80	"	2.18	13.87
1 month.....	912	2.22	17.22	"	2.30	17.62
2 months.....	748	"

CEMENT "X-55."

24 hours.....	375	2.537	7.90	610	2.569	8.30
2 days.....	440	2.441	9.18	630	2.527	9.80
4 days.....	509	2.617	9.50	615	2.542	11.40
8 days.....	635	2.410	11.35	620	2.430	10.60
16 days.....	754	2.290	12.86	674	2.480	12.15
1 month.....	782	2.120	14.00	705	2.530	10.65
2 months.....	658	686

CEMENT "119."

24 hours.....	328	2.581	7.55	615	2.501	10.00
2 days.....	713	2.39	11.18	576	2.46	12.68
4 days.....	795	2.45	12.25	722	2.41	11.10
8 days.....	815	2.39	12.35	555	2.49	12.30
16 days.....	796	2.30	12.85	570	2.46	12.35
1 month.....	688	2.12	13.11	721	2.42	13.60
2 months.....	685	705

TABLE III.

Cement from Mill No.	Autoclave Test, 24 hours; Tensile Strength, lb. per sq. in.	Autoclave Test, 18 months; Tensile Strength, lb. per sq. in.
1	689-800	750
	735	550
	620-612-595	590
	767-634-767	750
	777-730-668	596
	685-667-712	630
	545-698-654	650
	508-468-493	578-640
	575-659-553	600
	585-585-645	630-500
	649-639-631	574
	474-609-567	490-510
	540-492-510	550
	490-600-576	500
	725-797-645	550
	766-690-605	520-750
	disintegrated	300
2	328-188-100	420
	60-50	650
	550-541	661
	disintegrated	150
	191-166	600
	disintegrated	130
3	125-124-116	682
	448-448	520
	509-496	450
	278-330-308	416
	387-347-367	470
	508-496-500	700
	525-557-522	538-406-400
	445-403-424	672
	409-331-381	716
	418-461-566	520
	375-377	420
	465-437-486	650-498
	90-90	600
	590	430-416
4	192-178-175	300-150
	235-225-157	350-150
	582-581-497	600
	582	638
	781-753-725	728-624-712
	261-369	654-665
	457-672	650
	disintegrated	420
	"	350
	"	477
	"	350-300
5	"	457
	"	330
	64-67	245-235-240
	disintegrated	330
	"	376
	"	330
	568-510-604	567
	505-575-532	600-634-618
	481-474-580	482
	523-510-409	395-510
	133-340-349	300

Referring to Table II, briquettes placed in water showed that at the end of one month the "5-2000" cement does not pass the autoclave test, while cement "149" failed to pass the autoclave test at the end of 2 months. Cement "X 55" and "119" continued to pass the autoclave test, as shown in the table.

Table II clearly shows that neat cement mortar, though as small as 1 sq. in. in section, does not season when placed in water, even at the end of 2 months.

Samples of neat cement exposed to the atmosphere for 18 months (Table III) show in many cases that the cement does not season sufficiently to pass the autoclave test, although samples from some mills show considerable improvement in tensile strength. In some cases the improvement is so great that the cement at the end of this period meets the requirements of the autoclave test. This table also shows that the cement which had good tensile strength in the beginning maintains good tensile strength at the end of 18 months.

It is quite evident from Table III that there exists a great non-uniformity in Portland cement, and it is a question whether or not it is wise to place in the same work cement of various brands. At present we have nothing to show that any harm would come from using cement from different mills on the same job except the lack of uniformity as shown by the autoclave test.

DISCUSSION.

Mr. Wig. **MR. R. J. WIG.**—In order to determine the practical value of the accelerated tests for cement, we have been making at the Bureau of Standards some compression tests of concrete prepared with a number of cements. Some cements passed the autoclave test; other cements failed on this test, but passed the atmospheric steam test; and still others failed to pass either test, although they were normal in other respects. Cements of these various types were obtained from the same mill in a number of cases so as to eliminate as far as possible variation in chemical constituents and the method of manufacture. All these tests were made on 8 by 16-in. concrete cylinders, and specimens were made for testing over a period of ten years. Three specimens were made for each test period for each sample, and a total of 18 samples was included in this investigation.

Referring to Fig. 1, the results of tests of concretes made with cements which were unsound in the normal atmospheric steam test are plotted in the first or open column. In the second column are plotted the results of tests of concretes made with standard cement which passes the normal steam tests, but does not pass the autoclave test. In the third column are plotted the results of tests of concretes made with cements which pass both of these tests. Practically all cements passed all other tests except the steam or autoclave test. The letters in the figure indicate the various brands. Unfortunately, it was very difficult to obtain samples of some brands of cement which were unsound in the atmospheric steam test. Where the cement would be shipped from the mill as unsound in atmospheric steam, by the time it reached the laboratory it was often sound, so that it was included as a type No. 2 cement.

We find that there is practically no difference at 30 days between a standard cement and the autoclave cement, although the concrete made with unsound cement had an average compressive strength below either of the other types. While I state that there is no difference in results obtained with auto-

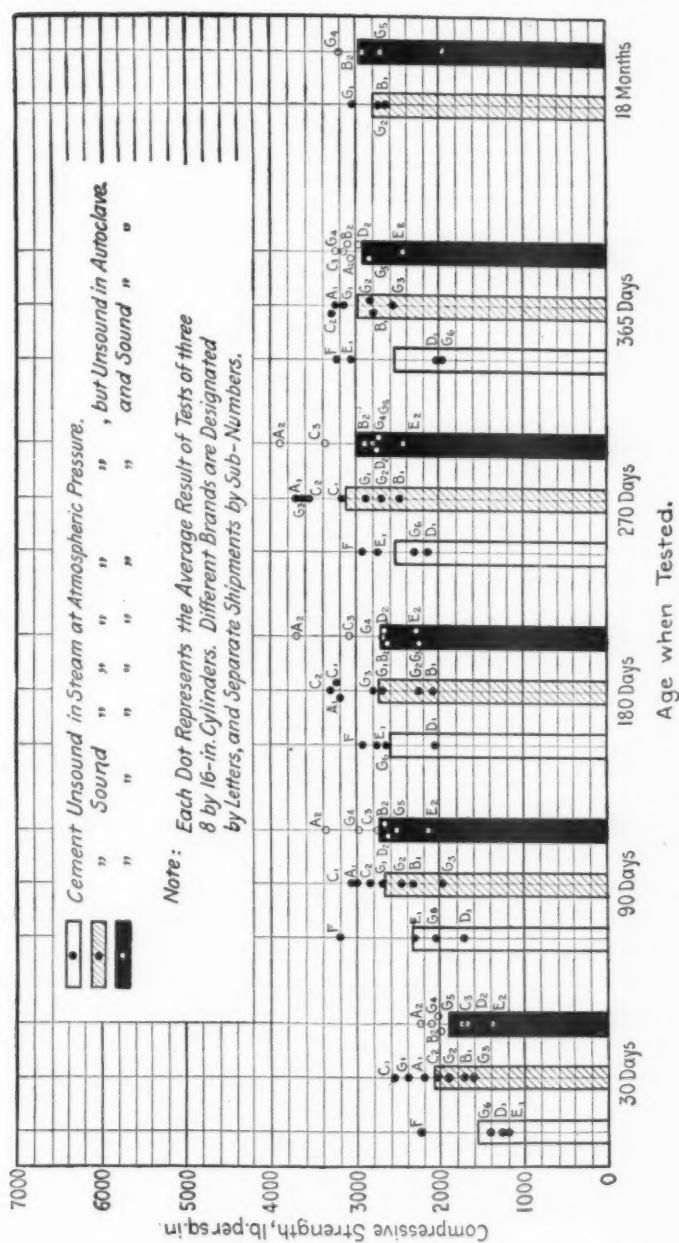


FIG. 1.—Results of Tests of Concrete.

Mr. Wig. clave and standard cements, there is a slight difference which may however be attributed to the variation due to the methods of molding the specimens. At 90 days there is less difference between the results with any of the different types of cement, showing that even the ordinary steam test is of little value in indicating the actual cementing value of cement. At 180 days, 270 days, and 365 days the results are similar, the average strength of the unsound cements being the lowest, and the standard and autoclave cements being about the same. At 18 months the autoclave is slightly higher than the others, although they are both practically the same in all these cases.

There is one other point of interest in connection with these results, namely, that every cement except one included in these tests shows retrogression at some period. We find in tracing the point of retrogression, that it always occurs in winter, regardless of when the concrete was fabricated. That is, if the concrete was molded in the spring, the retrogression would occur at the period of the test made during the winter months. Thus we find in the case of some cements that retrogression occurs in 90 days, in other cases in 270 days, and in still others it may not occur until the year test. This phenomenon has been observed before, but never quite so markedly as in these tests, and it suggests an investigation to determine the cause of this retrogression, which may also explain in part the retrogression reported by other laboratories on long-time tests of concrete.

In closing, I should like to ask Mr. Force one question in regard to his definition of the use of the term "boiling." He defines his test as a boiling test of cement. I should like to ask him if he includes under the boiling test the exposure of cement to steam above the water, or only the exposure of the cement immersed in the water. That is, would he define it as a test which might include both exposure above the water and immersion in the water?

Mr. Force. MR. H. J. FORCE.—In answer to Mr. Wig's question, we have a number of times made the test both in water and above water. As far as we are able to see, there is very little difference in the results. It was my intention to cover both tests, in the water and above the water, in the autoclave apparatus.

PROPORTIONING AGGREGATES FOR PORTLAND-CEMENT CONCRETE.

BY ALBERT MOYER.

SUMMARY

The object of the investigations into the proper proportioning of aggregates for Portland-cement concrete is to obtain maximum density which, other things being equal, the author considers gives maximum strength.

One of the principal results obtained in these investigations was that arbitrary specifications without previous knowledge of the character of the aggregates that are to be used are wrong, and that such stated proportions as 1 : 2 : 4 or 1 : 3 : 6, etc., are meaningless.

It was found that 94 lb. per cu. ft., or 3.8 cu. ft. per barrel, as a unit of measurement is incorrect. Investigations prove that it takes 110 lb. of Portland cement to make 1 cu. ft. of paste. The author finds that, opposed to the old plan of filling the voids in sand with cement, these voids must necessarily be filled with paste (cement and water), and that the study of the proportioning of aggregates must be based on the proper proportions of cement, water and sand to make a sufficiently rich mortar to bind together the larger aggregates. The cement must be measured by weight, using 110 lb. as equivalent to 1 cu. ft. of paste. If a 94-lb. bag is used as a unit for measurement, the proportions must be figured so that the same proportion of cement to sand is obtained as when based on 110 lb. of cement per cubic foot of paste.

For the purpose of economy, various sizes of stone should be used and investigation made to see which sizes will produce the least percentage of voids, so that less mortar may be used and more strength obtained, for the reason that mortar is not as

strong under compression as mortar and crushed stone or pebbles mixed together in the form of what we term concrete.

The paper gives various methods of carrying on investigations, so that with a given sand and a given stone or gravel, proportions can be stated by the engineer which will make a concrete of maximum density and maximum strength.

PROPORTIONING AGGREGATES FOR PORTLAND-CEMENT CONCRETE.

BY ALBERT MOYER.

To arrive at a scientific and at the same time practical method by which to properly proportion the aggregates for Portland-cement concrete, we must examine the larger available aggregates and so graduate them in size of particles that the smallest percentage of voids results and, consequently, that less mortar is required to obtain maximum density and therefore maximum strength. The principal requisite is the making of a sufficiently dense and rich mortar to bind together pebbles or crushed stone by entirely filling the voids. The study of this subject must be more particularly directed towards the proportioning of sand, Portland cement and water. The grains of sand must be bound together and the voids between the grains entirely filled with Portland-cement paste. It is the proportioning of this paste that we must study in order to determine the amount of cement and water which shall be added to any particular, available sand.

In figuring proportions, we must determine the amount of Portland-cement paste required to fill the voids in the sand. (It will be noted that Portland cement, dry, measured loosely, shrinks in volume when water is added, and when measured packed tightly, increases only slightly in volume when water is added. Therefore, we must consider the paste as being the element which must fill the voids in any given sand.) Experiments made by S. Warren Hartwell would indicate that 110 lb. of Portland cement mixed with 25 per cent of water are required to produce 1 cu. ft. of paste. The cement must be measured by weight and not by volume, for it has been found that 110 lb. of Portland cement will occupy 1.12 cu. ft. if measured loosely and dry, while if packed tightly it will occupy only 0.96 cu. ft. If 94 lb. of Portland cement is figured as 1 cu. ft., there is not enough cement in the concrete.

PROPORTIONING THE MORTAR.

There are various methods of determining the amount of Portland-cement paste required to fill the voids in the sand, among which are (1) the determination of the percentage of voids by the specific-gravity method, and (2) by the water test (that is, the volume of water required to fill the voids); (3) experiments to determine directly the required amount of paste by measuring the volumes of mortars of various proportions, and (4) making up the mortars into briquettes and breaking them in 7 and 28-day periods.

1. *Specific Gravity Method.*—To be accurate, the specific gravity of the sand used should first be determined. Then weigh a given quantity of sand, compute the volume it would occupy if solid, and thus obtain the percentage of voids. It is fairly accurate, however, to assume that the specific gravity of a well-washed quartz sand is 2.65, and to base the computations for the percentage of voids in sands on this figure.

2. *Water Test.*—The water test for voids in sand is made as follows: Provide two 250-cc. graduated glasses. Dry the sample of sand to be tested by spreading a thin layer on a pan or piece of tin and heating to a temperature of over 212° F.; this will determine as nearly as possible an accurate unit of measurement. When the sand is cool, pour 100 cc. slowly into a graduated glass, jarring the glass by tapping on the bottom and sides while pouring; this assists in expelling the air. Level off the top with a flat-end stick, in order that the reading to 100 cc. may be accurately made.

In the other glass, measure out 100 cc. of water, pour the dry sand slowly into the glass containing the water, tapping the glass while pouring so as to expel as much of the air as possible. Note the height to which the water rises. If 100 cc. of solid matter had been placed in the glass, the water would have risen to the 200-cc. mark; therefore to ascertain the percentage of voids, deduct the number of cubic centimeters to which the water has risen from 200.

3. *Experiments with Mortars of Various Proportions.*—Provide several graduated glasses which will contain 250 cc. or over; also provide a balance which will weigh accurately

to hundredths of an ounce. Pouring 200 cc. of dry sand into a glass very slowly, tapping the bottom and sides so that it will settle in as dense a mass as possible. Then pour the sand out on the balance and determine its weight to a hundredth of an ounce. Measure out several more 200-cc. samples of sand, weighing each time as a check on the quantity of sand in each sample. Then determine the volumes of cement paste required to be mixed with the 200-cc. samples of sand to give the various proportions under investigation, and measure out the necessary

TABLE I.—WEIGHT OF CEMENT REQUIRED FOR A GIVEN VOLUME OF CEMENT PASTE.

1 CU. FT. OF PASTE CONTAINS 110 LB. OF CEMENT; THEREFORE 100 CC. OF PASTE CONTAINS 6.22 OZ. OF CEMENT.

Proportions by Volume.	Volume of Cement Paste, cc.	Volume of Sand, cc.	Weight of Cement Required, oz.
1 : $1\frac{1}{2}$	133	200	8.27
1 : $1\frac{3}{4}$	115	200	7.16
1 : 2	100	200	6.22
1 : $2\frac{1}{4}$	89	200	5.53
1 : $2\frac{1}{2}$	80	200	4.97
1 : $2\frac{3}{4}$	73	200	4.55
1 : 3	66	200	4.11
1 : $3\frac{1}{4}$	62	200	3.86
1 : $3\frac{1}{2}$	57	200	3.55
1 : $3\frac{3}{4}$	53	200	3.30
1 : 4	50	200	3.11

amount of cement by weight, assuming the weight of cement to be 110 lb. per cu. ft. These data, for various proportions, are given in Table I. The following proportions, by volume, of cement paste to sand are suggested for investigation: 1 : $1\frac{1}{2}$, 1 : 2, 1 : $2\frac{1}{4}$, 1 : $2\frac{1}{2}$, 1 : $2\frac{3}{4}$, and 1 : 3.

Mix each sample of cement with 25 per cent of water. Then mix, first, the sample for the richest mixture with a 200-cc. sample of sand, and tamp all of the resulting mortar into the graduated glass, a little at a time, with a flat-end stick. Note the volume occupied by the tamped mortar. Similarly for the

next richest mixture, and so on until all the samples have been mixed into mortar and placed in the glasses.

The object of this test is to ascertain what proportion with a given amount of sand will produce maximum density with a minimum amount of cement. Since the same amount of sand is used in each sample, it is evident that with too much cement the volume of the mortar will be increased. Therefore, in progression from the leanest to the richest mixture, that sample which first starts to increase the volume of the mortar is the one which contains the correct proportions for that particular sand.

4. *Tests of Briquettes.*—The reason for using 200 cc. of sand in the previous experiment is to obtain sufficient mortar to make three briquettes. By inverting the glass and jarring the top against a rigid surface, the mortar will be released, usually in a solid mass. Make up each sample of mortar into briquettes, store in a moist closet, and break in 7 days.

A better test is to make up 2-in. cubes and crush at 7 and 28-day periods.

This strength test is a check upon the accuracy of the above tests for the proportions of cement to sand which will produce maximum density, maximum strength and maximum bonding. The results of the four tests should agree. If they do not, the tests should be made again. If there is still a considerable variation, wash the sand and retest.

To illustrate the reliability of the four methods just described: A test was made of a sample of Rockaway sand which is found along the south shore of Long Island. It was found that 100 cc. of sand, packed tightly in a graduated glass, weighed 5.875 oz. When placed in 100 cc. of water, the water rose in the graduated glass to 162 cc. This would indicate 38 per cent of voids. (This sand was so fine that 66.9 per cent passed through a No. 50 sieve and 98.3 per cent passed through a No. 30 sieve.) If the specific gravity of Rockaway sand is 2.65,—and it undoubtedly appears to be a quartz sand,—the calculated voids are then 37.2 per cent, which checks up the water test. Therefore, the proportions for maximum density, based on 38 per cent of voids, are 1 part of cement paste to $2\frac{1}{2}$ parts of sand.

Briquettes of mortar made of this sand and Portland cement in varying proportions were tested, with the following results:

PROPORTIONS.	TENSILE STRENGTH IN 28 DAYS,	
	LB. PER SQ. IN.	
1 : 2 $\frac{1}{4}$	282	
1 : 2 $\frac{1}{2}$	334	
1 : 2 $\frac{3}{4}$	367	
1 : 2.....	396	

It would appear that the proportion giving excellent strength is either 1 : 2 $\frac{1}{2}$ or 1 : 2 $\frac{3}{4}$, which checks up the water test and the specific-gravity test, for 1 : 2 is unnecessarily strong. The proportion 1 : 2 $\frac{1}{4}$ is considerably weaker than the 1 : 2 $\frac{1}{2}$, although this sand could be used with safety in proportions of 1 : 2 $\frac{3}{4}$ provided the mortar was rich enough in cement to properly bond together the larger sizes of crushed stone. It is evident, however, that the voids in the 1 : 2 $\frac{3}{4}$ mixture have not been filled, and therefore the mortar would be too short, while the 1 : 2 $\frac{1}{2}$ or 1 : 2 $\frac{1}{4}$ mixture is sufficiently rich and strong. These data bear out the correctness of the methods used in obtaining the percentage of voids.

Therefore, in using a sand in which there are found to be 38 per cent of voids, the proportion would be 110 lb. of cement to 2 $\frac{1}{2}$ cu. ft. of sand, or 1 cu. ft. of cement paste to 2 $\frac{1}{2}$ cu. ft. of sand. If it is necessary to use the bag (94 lb.) as the unit for measuring the cement, the proportions should be computed as follows:

94 lb. : 110 lb. = 38 per cent of voids : x per cent of voids,
from which $x = 44$ per cent, which calls for a proportion of 1 : 2 $\frac{1}{4}$. Therefore, using the bag as a unit, the proportions will be 1 bag of cement (94 lb.) to 2 $\frac{1}{4}$ cu. ft. of sand. It is certainly impracticable to change the size and weight of the package, and it is convenient to use the package as a unit for measurement. Therefore, the actual percentage of voids in the sand should first be calculated and then arbitrarily increased by means of the above method.

If screenings or quarry tailings passing through a $\frac{1}{4}$ -in.-mesh sieve are used in place of sand, their voids are determined in the same manner as those of sand, previously described.

The tailings, however, should be of such size that not more than 20 per cent passes through a No. 50 sieve and not more than 10 per cent through a No. 100 sieve. A sample of such screenings taken from Snake Hill in the middle of the Hackensack Meadows, N. J., and passed through a $\frac{1}{4}$ -in. sieve, analyzed as follows:

Retained on No. 10 Sieve.....	47.5 per cent
" " " 20 ".....	22.5 "
" " " 30 ".....	8.8 "
" " " 50 ".....	9.3 "
Passing " 50 ".....	11.9 "
	<hr/>
	100.0 "

The percentage of voids was found by the water test to be 34.

Trap-rock screenings make a very excellent substitute for sand; in fact, they will make a far tougher and stronger concrete than the best sand. Poor stone screenings, however, such as soft limestone, marble, sandstone, etc., do not make as good a mortar for concrete as does good sand.

PROPORTIONING THE LARGER AGGREGATES.

Let us now consider the proportioning of the larger aggregates, keeping in mind that the voids in these aggregates are to be filled with mortar as previously described. It is sometimes best to use two sizes of stone, a large size and a smaller size; the smaller size, however, should all be retained on a $\frac{1}{4}$ -in.-mesh sieve.

In order to ascertain the best mixture of the smaller and larger sizes of stone or pebbles, make a receptacle which will hold a little over 4 cu. ft. (or use a 15-in. sewer pipe). Measure 3 cu. ft. of the larger stone and 1 cu. ft. of the smaller stone. Mix well together, place in the receptacle and note the space which it occupies. Empty the receptacle, measure 2 cu. ft. of the larger stone and 2 cu. ft. of the smaller stone, mix as before and mark the space occupied. Vary the proportions in this manner, always adhering to a total of 4 cu. ft. The mixture which occupies the least space in the receptacle will make the densest concrete.

TABLE II.—PERCENTAGE OF VOIDS IN CRUSHED STONE.

Average Specific Gravity.	2.70	2.85	2.40	2.65	2.80	3.03	2.14	2.60	3.25
Weight per Cubic Foot of Crushed Stone, lb.	Hard Calcite; Limestone; Hard Marble; Granite.	Dolomite; Hard Limestone.	Medium-Hard Limestone; Medium Sandstone.	Quartzite; Quartz Pebbles; Quartz Gravel.	Boston Trap; Hard Sandstone.	Hudson Co. Trap.	Rockland Co., N. Y., and Ohio Sandstone.	Flint; Hard Feldspar.	Hornblende.
80.....	52.5	55.0	46.5	51.6	54.2	57.6	40.0	50.6	60.5
81.....	51.9	54.4	45.8	51.0	53.6	57.1	39.3	50.0	60.0
82.....	51.3	53.8	45.2	50.4	53.0	56.6	38.5	49.4	59.5
83.....	50.7	53.3	44.5	49.8	52.4	56.1	37.8	48.8	59.0
84.....	50.1	52.7	43.8	49.1	51.9	55.5	37.0	48.2	58.5
85.....	49.5	52.1	43.2	48.5	51.3	55.0	36.3	47.6	58.0
86.....	48.9	51.6	42.5	47.9	50.7	54.5	35.5	46.9	57.5
87.....	48.3	51.0	41.8	47.3	50.1	53.9	34.8	46.3	57.0
88.....	47.7	50.5	41.2	46.7	49.6	53.4	34.0	45.7	56.5
89.....	47.1	49.9	40.5	46.1	49.0	52.9	33.3	45.1	56.1
90.....	46.5	49.3	39.8	45.5	48.4	52.3	32.5	44.5	55.6
91.....	45.9	48.8	39.2	44.9	47.8	51.8	31.8	43.8	55.1
92.....	45.3	48.2	38.5	44.3	47.3	51.3	31.0	43.2	54.6
93.....	44.7	47.6	37.8	43.7	46.7	50.8	30.3	42.6	54.1
94.....	44.1	47.1	37.2	43.1	46.1	50.2	29.5	42.0	53.6
95.....	43.5	46.5	36.5	42.5	45.6	49.7	28.9	41.4	53.1
96.....	43.0	45.9	35.8	41.9	45.0	49.2	28.0	40.8	52.6
97.....	42.4	45.4	35.2	41.3	44.4	48.6	27.3	40.1	52.1
98.....	41.8	44.8	34.5	40.7	43.8	48.1	26.5	39.5	51.6
99.....	41.2	44.3	33.8	40.1	43.3	47.6	..	38.9	51.1
100.....	40.6	43.7	33.1	39.5	42.7	47.1	..	38.3	50.6
101.....	40.0	43.1	32.5	38.8	42.1	46.5	..	37.7	50.1
102.....	39.4	42.6	31.8	38.2	41.6	46.0	..	37.1	49.6
103.....	38.8	42.0	31.1	37.6	41.0	45.5	..	36.4	49.1
104.....	38.2	41.4	30.5	37.0	40.4	44.9	..	35.8	48.6
105.....	37.6	40.9	29.8	36.4	39.8	44.4	..	35.2	48.2
106.....	37.0	40.3	29.1	35.8	39.3	43.9	..	34.6	47.7
107.....	36.4	39.8	28.5	35.2	38.7	43.3	..	34.0	47.2
108.....	35.8	39.2	27.8	34.6	38.1	42.8	..	33.4	46.7
109.....	35.2	38.6	27.1	34.0	37.5	42.3	..	32.7	46.2
110.....	34.6	38.1	26.5	33.4	37.0	41.8	..	32.1	45.7
111.....	34.0	37.5	..	32.8	36.4	41.2	..	31.5	45.2
112.....	33.4	36.9	..	32.2	35.8	40.7	..	30.9	44.7
113.....	32.8	36.4	..	31.6	35.3	40.2	..	30.3	44.2
114.....	32.3	35.8	..	31.0	34.7	39.6	..	29.7	43.7
115.....	31.7	35.3	..	31.4	34.1	39.1	..	29.0	43.2
116.....	31.1	34.7	..	29.8	33.5	38.6	..	28.4	42.7
117.....	30.5	34.1	..	29.2	32.9	38.0	..	27.8	42.2
118.....	29.9	33.6	..	28.6	32.4	37.5	..	27.2	41.7
119.....	29.3	33.0	..	28.0	31.8	37.0	..	26.6	41.2
120.....	28.7	32.4	..	27.3	31.2	36.5	40.8
121.....	28.1	31.9	..	26.7	30.7	35.9	40.3
122.....	27.5	31.3	30.1	35.4	39.8
123.....	26.9	30.7	29.5	34.9	39.3
124.....	..	30.2	28.9	34.3	38.8

To ascertain the percentage of voids in crushed stone or pebbles, make a box of 3-cu.-ft. capacity, the box to be say 1 by $1\frac{1}{2}$ by 2 ft. Dry the sand or gravel, heating to over 212° F. Weigh the box empty; throw the stone into the box loose, level off the top with a straight edge, and weigh. Deduct the weight of the empty box from the gross weight and divide the net weight by 3, which will give the actual weight of 1 cu. ft. The percentage of voids may then be obtained by referring to Table II, which is based upon the average specific gravities of the stones tabulated.

In determining the percentage of voids it is essential that the stone shall contain no screenings and shall be free from sand. If any of the particles will pass through a $\frac{1}{4}$ -in.-mesh sieve they should be screened out and figured as sand.

The percentage of voids being known, the parts of stone required for various proportions of mortar to fill the voids can now be found. These have been tabulated in Table III.

For example: Suppose that the sand or screenings selected for use requires a proportion of 1 part of cement and $2\frac{1}{2}$ parts of sand to produce maximum density of mortar and sufficient richness for a good bond, and that the stone at hand is a hard crushed granite of 1-in. size, graduated in particles, and weighing 100 lb. per cu. ft. From Table II we find the percentage of voids in the larger aggregates to be 43. Referring to Table III it will be seen that the proportions for maximum density in such concrete will be 1 part of Portland-cement paste (110 lb. of cement for 1 cu. ft. of paste), $2\frac{1}{2}$ parts of sand and $5\frac{3}{4}$ parts crushed granite; or stated in another way, 110 lb. of Portland cement, $2\frac{1}{2}$ cu. ft. of sand and $5\frac{3}{4}$ cu. ft. of stone.

A practical field test may be made by providing 4 lengths of 15-in. sewer pipe and actually placing concrete, using the same amount of stone or pebbles in each instance but varying the proportions of mortar, the proportions of sand and cement having been previously determined by methods above described.

Make up four batches of concrete as follows: If the proportions of mortar to crushed stone as ascertained by methods previously described are for instance $2\frac{1}{2}$ parts mortar, 5 parts stone, adhere to the same quantity of stone in each sample,

TABLE III.—PROPORTIONS OF STONE EXPRESSED IN CUBIC FEET.

Voids in Stone, per cent.	Proportions of Mortar.										
	1:1 $\frac{1}{2}$	1:1 $\frac{3}{4}$	1:2	1:2 $\frac{1}{4}$	1:2 $\frac{1}{2}$	1:2 $\frac{3}{4}$	1:3	1:3 $\frac{1}{4}$	1:3 $\frac{1}{2}$	1:3 $\frac{3}{4}$	1:4
27	5 $\frac{1}{2}$	6 $\frac{1}{2}$	7 $\frac{1}{2}$	8 $\frac{1}{4}$	9 $\frac{1}{4}$	10	11	12	13	14	14 $\frac{3}{4}$
28	5 $\frac{3}{4}$	6 $\frac{1}{4}$	7 $\frac{1}{4}$	8	9	9 $\frac{3}{4}$	10 $\frac{3}{4}$	11 $\frac{1}{2}$	12 $\frac{1}{2}$	13 $\frac{1}{2}$	14 $\frac{1}{4}$
29	5 $\frac{1}{4}$	6	7	7 $\frac{3}{4}$	8 $\frac{1}{2}$	9 $\frac{1}{2}$	10 $\frac{1}{2}$	11 $\frac{1}{4}$	12	13	13 $\frac{3}{4}$
30	5	5 $\frac{3}{4}$	6 $\frac{3}{4}$	7 $\frac{1}{2}$	8 $\frac{1}{4}$	9 $\frac{1}{4}$	10	10 $\frac{3}{4}$	11 $\frac{3}{4}$	12 $\frac{1}{2}$	13 $\frac{1}{4}$
31	5	5 $\frac{3}{4}$	6 $\frac{1}{2}$	7 $\frac{1}{4}$	8	9	9 $\frac{3}{4}$	10 $\frac{1}{2}$	11 $\frac{1}{4}$	12	13
32	4 $\frac{3}{4}$	5 $\frac{1}{2}$	6 $\frac{1}{4}$	7	7 $\frac{3}{4}$	8 $\frac{1}{2}$	9 $\frac{1}{4}$	10	11	11 $\frac{3}{4}$	12 $\frac{1}{2}$
33	4 $\frac{1}{2}$	5 $\frac{1}{4}$	6	6 $\frac{3}{4}$	7 $\frac{1}{2}$	8 $\frac{1}{4}$	9	9 $\frac{3}{4}$	10 $\frac{3}{4}$	11 $\frac{1}{2}$	12
34	4 $\frac{1}{2}$	5 $\frac{1}{4}$	6	6 $\frac{1}{2}$	7 $\frac{1}{4}$	8	8 $\frac{3}{4}$	9 $\frac{1}{2}$	10 $\frac{1}{4}$	11	11 $\frac{3}{4}$
35	4 $\frac{1}{4}$	5	5 $\frac{3}{4}$	6 $\frac{1}{2}$	7 $\frac{1}{4}$	7 $\frac{3}{4}$	8 $\frac{1}{2}$	9 $\frac{1}{4}$	10	10 $\frac{3}{4}$	11 $\frac{1}{2}$
36	4 $\frac{1}{4}$	5	5 $\frac{1}{2}$	6 $\frac{1}{4}$	7	7 $\frac{3}{4}$	8	9	9 $\frac{3}{4}$	10 $\frac{1}{2}$	11
37	4	4 $\frac{3}{4}$	5 $\frac{1}{4}$	6	6 $\frac{3}{4}$	7 $\frac{1}{2}$	8	8 $\frac{3}{4}$	9 $\frac{1}{2}$	10 $\frac{1}{4}$	10 $\frac{3}{4}$
38	4	4 $\frac{3}{4}$	5 $\frac{1}{2}$	6	6 $\frac{1}{2}$	7 $\frac{1}{4}$	8 $\frac{1}{4}$	8 $\frac{1}{2}$	9 $\frac{1}{4}$	10	10 $\frac{1}{2}$
39	4	4 $\frac{1}{2}$	5	5 $\frac{3}{4}$	6 $\frac{1}{4}$	7	7 $\frac{3}{4}$	8 $\frac{1}{4}$	9	9 $\frac{3}{4}$	10 $\frac{1}{4}$
40	3 $\frac{3}{4}$	4 $\frac{1}{2}$	5	5 $\frac{3}{4}$	6 $\frac{1}{4}$	7	7 $\frac{1}{2}$	8	8 $\frac{3}{4}$	9 $\frac{1}{2}$	10
41	3 $\frac{3}{4}$	4 $\frac{1}{4}$	4 $\frac{3}{4}$	5 $\frac{1}{2}$	6	6 $\frac{3}{4}$	7 $\frac{1}{4}$	8	8 $\frac{1}{2}$	9 $\frac{1}{4}$	9 $\frac{3}{4}$
42	3 $\frac{1}{2}$	4 $\frac{1}{4}$	4 $\frac{3}{4}$	5 $\frac{1}{2}$	6	6 $\frac{1}{2}$	7 $\frac{1}{4}$	7 $\frac{3}{4}$	8 $\frac{1}{4}$	9	9 $\frac{1}{4}$
43	3 $\frac{1}{2}$	4	4 $\frac{3}{4}$	5 $\frac{1}{4}$	5 $\frac{3}{4}$	6 $\frac{1}{2}$	7	7 $\frac{1}{2}$	8 $\frac{1}{4}$	8 $\frac{3}{4}$	9 $\frac{1}{4}$
44	3 $\frac{1}{2}$	4	4 $\frac{1}{2}$	5 $\frac{1}{4}$	5 $\frac{3}{4}$	6 $\frac{1}{4}$	6 $\frac{3}{4}$	7 $\frac{1}{2}$	8	8 $\frac{1}{2}$	9
45	3 $\frac{1}{4}$	4	4 $\frac{1}{2}$	5	5 $\frac{1}{2}$	6	6 $\frac{3}{4}$	7 $\frac{1}{4}$	7 $\frac{3}{4}$	8 $\frac{1}{4}$	8 $\frac{3}{4}$
46	3 $\frac{1}{4}$	3 $\frac{3}{4}$	4 $\frac{1}{4}$	5	5 $\frac{1}{2}$	6	6 $\frac{1}{2}$	7	7 $\frac{3}{4}$	8 $\frac{1}{4}$	8 $\frac{3}{4}$
47	3 $\frac{1}{4}$	3 $\frac{3}{4}$	4 $\frac{1}{4}$	4 $\frac{3}{4}$	5 $\frac{1}{4}$	5 $\frac{3}{4}$	6 $\frac{1}{4}$	7	7 $\frac{1}{2}$	8	8 $\frac{1}{2}$
48	3 $\frac{1}{4}$	3 $\frac{3}{4}$	4	4 $\frac{3}{4}$	5 $\frac{1}{4}$	5 $\frac{3}{4}$	6 $\frac{1}{4}$	6 $\frac{3}{4}$	7 $\frac{1}{4}$	7 $\frac{3}{4}$	8 $\frac{1}{4}$
49	3	3 $\frac{1}{2}$	4	4 $\frac{1}{2}$	5	5 $\frac{1}{2}$	6 $\frac{1}{4}$	6 $\frac{3}{4}$	7	7 $\frac{3}{4}$	8 $\frac{1}{4}$
50	3	3 $\frac{1}{2}$	4	4 $\frac{1}{2}$	5	5 $\frac{1}{2}$	6	6 $\frac{1}{2}$	7	7 $\frac{1}{2}$	8
51	3	3 $\frac{1}{2}$	3 $\frac{3}{4}$	4 $\frac{1}{2}$	5	5 $\frac{1}{2}$	6	6 $\frac{1}{2}$	6 $\frac{3}{4}$	7 $\frac{1}{4}$	7 $\frac{3}{4}$
52	3	3 $\frac{1}{4}$	3 $\frac{3}{4}$	4 $\frac{1}{4}$	4 $\frac{3}{4}$	5 $\frac{1}{4}$	5 $\frac{3}{4}$	6 $\frac{1}{4}$	6 $\frac{3}{4}$	7 $\frac{1}{4}$	7 $\frac{3}{4}$
53	3	3 $\frac{1}{4}$	3 $\frac{3}{4}$	4 $\frac{1}{4}$	4 $\frac{3}{4}$	5 $\frac{1}{4}$	5 $\frac{3}{4}$	6 $\frac{1}{4}$	6 $\frac{3}{4}$	7	7 $\frac{1}{2}$
54	3	3 $\frac{1}{4}$	3 $\frac{3}{4}$	4 $\frac{1}{4}$	4 $\frac{3}{4}$	5	5 $\frac{1}{2}$	6	6 $\frac{1}{2}$	7	7 $\frac{1}{4}$

using $2\frac{1}{2}$ cu. ft. of stone as a matter of convenience for each batch of concrete.

Make up one batch of $1\frac{1}{8}$ cu. ft. of mortar, $2\frac{1}{2}$ cu. ft. of stone; another batch $1\frac{1}{4}$ cu. ft. of mortar, $2\frac{1}{2}$ cu. ft. of stone; another batch $1\frac{3}{8}$ cu. ft. of mortar, $2\frac{1}{2}$ cu. ft. of stone; and the fourth batch $1\frac{1}{2}$ cu. ft. of mortar, $2\frac{1}{2}$ cu. ft. of stone. Since the same amount of stone is used in each sample, it is evident that with too much mortar the volume of the concrete will be increased, therefore in progression from the leanest to the richest mixture, that sample which first starts to increase the volume of the concrete is the one which contains the correct proportions.

It may reasonably be stated, therefore, that aggregates proportioned as above described produce a concrete which is denser and stronger than aggregates not so proportioned. Thus, with a given aggregate and an arbitrary specification of 1 : 2 : 4, a weaker concrete might be obtained than if the aggregates were properly proportioned in accordance with the principles above described and the proper proportions proved to be, for example, $1 : 2\frac{1}{2} : 4\frac{3}{4}$.

DISCUSSION.

MR. CLOYD M. CHAPMAN.—Before entering upon any discussion of this paper I want to express my high appreciation of the work done in this particular field by the author of the paper, and to emphasize the importance of just such work as the paper shows has been done. It is only by exploring the field of possible methods and by trying them out and discussing them pro and con, with the data and results before us, that progress can be made toward the development and adoption of tests which will completely fulfil the many and varied conditions of the case. Mr. Chapman.

As a broad proposition it is probably true that the densest concrete is the strongest, and that the determination of voids in fine and coarse aggregate by the various methods given in the paper will assist in proportioning a concrete of maximum density. But in any given case the engineer is not dealing with broad conditions. He is dealing with the particular materials available at or comparatively near the site of the proposed work. These materials may or may not be of an unusual character. Whatever the method employed for determining the proportions to be used in the concrete, it should be one which applies to all cases and to that particular one especially.

The author states that the proportion of cement to be added to a sand may be determined by the simple method previously described. But sometimes this fails to work. We have tested a sand which gave the following resulting volumes expressed as a percentage of volume of sand used, when mixed with various proportions of cement:

PROPORTIONS.	RESULTING VOLUMES.
1 : 2.....	126 per cent
1 : 2½.....	120 "
1 : 3.....	114 "
1 : 3½.....	112 "
1 : 4.....	110 "

Mr. Chapman.

According to these results the 1 : 4 mixture ought to be rich enough, yet a 1 : 3 mixture gave tensile strengths as compared with Ottawa sand as follows:

At 7 days.....	47 per cent
At 28 "	50 "

This is hardly a suitable mix for good concrete work.

The statement is made that not over 10 per cent of a tailings sand should pass a 100-mesh sieve. We have a record of a test of such a sand of which 18.8 per cent passed a 100-mesh sieve, yet it gave tensile strengths compared with a 1 : 3 mix with Ottawa sand as follows:

At 3 days.....	159 per cent
At 7 "	166 "
At 28 "	174 "

In compression the following strengths at 28 days were obtained:

PROPORTIONS.	COMPRESSIVE STRENGTH, LB. PER SQ. IN.
1 : 2.....	4250
1 : 2½.....	3500
1 : 3.....	3000
1 : 4.....	2750
1 : 5.....	2000

This can hardly be called a poor sand, yet it would be rejected under such a specification. Another good sand had 21.7 per cent passing through a 100-mesh sieve.

The author advocates the tension test of briquettes as one of the four most important tests to be used in determining the value of a sand. As an illustration of how far astray the tension test may lead one, two cases may be cited.

One sand gave tensile strengths, compared with Ottawa sand, as follows:

At 3 days.....	78.8 per cent
At 7 "	104.0 "
At 28 "	109.0 "

In compression with 2-in. cubes, at 28 days, the following:

Mr. Chapman.

PROPORTIONS.	COMPRESSIVE STRENGTH, LB. PER SQ. IN.
1 : 2.....	6750
1 : 2½.....	5750
1 : 3.....	5250
1 : 4.....	3750
1 : 5.....	2500

The other sand gave tensile strength ratios as follows:

At 3 days.....	103 per cent
At 7 ".....	104 "
At 28 ".....	100 "

These values are not materially less than the other sand. But in compression at 28 days it gave the following:

PROPORTIONS.	COMPRESSIVE STRENGTH, LB. PER SQ. IN.
1 : 2.....	3875
1 : 2½.....	3500
1 : 3.....	2500
1 : 4.....	2000
1 : 5.....	1750

In other words, one sand was as strong in compression in a 1 : 4 mixture as the other was in a 1 : 2½ mixture, although only a few per cent stronger in tension.

The tension test is a risky guide in judging a material to be used in compression. We do not use it in the case of other structural materials which are used only in compression, and why stick so doggedly to it in the case of mortar? How popular would a tension test for brick or hollow tile be among structural engineers?

A claim has been made by some that the percentage of water required to make normal consistency with the sand under test may be a reliable guide to its strength qualities,—the more water required, the poorer the sand. Here is a sand, however, which required 33 per cent more water than did standard Ottawa sand with the same cement, yet the tensile strength of the 1 : 3 mix at 28 days was 196 per cent of that of Ottawa sand. The rule does not always work, and I fear the exceptions are so numerous as to cast a doubt on the value of the rule.

Mr. Chapman.

The author infers that the proper amount of cement to use with a sand to give maximum density and maximum strength is directly proportional to the volume of the voids. This would be true if the object of adding the cement were simply to fill the voids. With some sands it is not enough that the voids are filled with cement. Some sands having a relatively low percentage of voids make weaker mortar than others having relatively high percentages of voids. A certain cement having 40 per cent of voids, by the displacement method, shows a tensile strength of 115 per cent of that of Ottawa sand at 28 days, while another sample having but 35 per cent of voids shows a strength of only 86.4 per cent at the same age. The object of adding cement to sand is usually to produce the desired strength. So many conditions affect the strength of the mixture that it is unsafe to conclude that if we know the amount of cement required to fill the voids in a given sand, we can then decide what mix to use in concrete. The only safe way, in my opinion, is to make up test specimens of a progressive series of mixtures of the materials to be used on the job, including the coarse aggregate, sand, cement and the *water*, and test these specimens in compression.

The question which the engineer on the job must answer is not "How much cement will fill the voids in sand, and how much mortar must I add to stone to fill its voids," but he must be able to answer most positively the question, "In what proportion must I mix this stone, sand, cement, and water to produce a concrete which will have the strength required in the structure with a proper factor of safety?"

With certain kinds of sand and stone, for instance, a clean coarse sand and crushed trap rock, the method described by the author would probably give satisfactory results, but if it is depended upon as a universal rule, somebody very soon is liable to put in some very poor concrete.

I wish, again, to compliment Mr. Moyer on the work he has done. The tests he advocates give satisfactory results in a majority of cases. These tests have been among those applied to concrete aggregates in the laboratory of Westinghouse, Church, Kerr & Co. for several years. All sands tested are subjected to these tests, except that instead of making briquettes for

tension tests of the mortar resulting from the "increase-in-volume" test, we make 2-in. cubes and crush them at the end of 7 and 28 days. But we have tested so many sands which did not follow the general rule that it seems unsafe to depend upon any or all of these tests, when final decision is made. Mr. Chapman.

MR. W. M. KINNEY.—Mr. Moyer's paper presents some very interesting material on a subject which has long received too little attention from engineers and architects specifying concrete for various uses. When we stop to consider that there is frequently a range of 500 lb. either way from the commonly accepted standard of 2000 lb. per sq. in. in compression, depending upon whether one or another of the local materials available is used, and that an equally great improvement in the strength may be made by varying the ratio of fine and coarse aggregate, while still maintaining the same ratio of cement to volume of concrete produced, it is surprising to note that but very little attention has been given to the subject of scientific proportioning of concrete to produce the greatest strength for the least outlay of money. There is no doubt that a more thorough study of the subject would lead to the production of far more suitable aggregate for concrete, placing as it would an actual demand on the producer which would have to be met. It is not uncommon to find that when the specifications distinctly state that the fine aggregate shall pass a $\frac{1}{4}$ -in. mesh, and be graded from fine to coarse, and that the coarse aggregate shall be well graded material from $\frac{1}{4}$ to $1\frac{1}{2}$ in. and the proportions based on these sizes, the producer will be screening over a $\frac{3}{16}$, $\frac{3}{4}$ and $1\frac{1}{2}$ -in. circular screen. It is common practice to accept the apparent inevitable, using the material passing the $\frac{3}{16}$ -in. screen as fine aggregate, the $\frac{3}{16}$ to $\frac{3}{4}$ -in. material as coarse aggregate for reinforced concrete work and the $\frac{3}{4}$ to $1\frac{1}{2}$ -in. material as coarse aggregate for mass work. Such conditions arise largely through ignorance of what is desired rather than through any objection on the part of the producer to giving the right kind of material. To him sand is sand, gravel is gravel, and crushed stone is crushed stone. It is up to the user to know what he wants and to get it. Mr. Kinney.

In this connection, the speaker is familiar with a series of tests conducted by Mr. F. M. McCullough of the Carnegie Technical School, in which the ratio of cement to fine plus coarse

Mr. Kinney. aggregate was kept constant at 1 to 6. The mixture of one part cement, one part sand and five parts gravel, gave considerably higher results than the 1 : 2 : 4 mixture, largely due to the fact that the gravel contained a high percentage of material passing the $\frac{1}{4}$ -in. mesh, which should have been classed as sand, when the commonly accepted proportions of 1 : 2 : 4 were used. Had the material been properly screened, this condition would not have held, but the tests were made on average samples of sand and gravel commonly sold in Pittsburgh for reinforced-concrete work, where most specifications for this class of work call for a 1 : 2 : 4 mixture. Arbitrary proportions calling for a ratio of fine aggregate to coarse aggregate of 1 : 2 are satisfactory, providing that the other conditions are maintained, namely, (1) that the dividing line between fine and coarse be the $\frac{1}{4}$ -in. sieve and all aggregate passing that screen be considered as fine aggregate, and (2) that the coarse aggregate be a graded mixture from $\frac{1}{4}$ to $1\frac{1}{4}$ in. or $1\frac{1}{2}$ in., and not a product of practically uniform size. The natural grading of these materials, however, is not exact, and great improvement can be effected by more accurate and careful proportioning.

There is no doubt that density proportioning is the most practical and definite method yet evolved. While it is largely a cut-and-try method and should be checked by cylinder compression tests, there are fewer possibilities of error, and the results are not dependent on the use of delicate apparatus. The value of this test in proportioning, however, is not as great as would be indicated by Mr. Moyer's experiments, inasmuch as usually concrete of a certain strength is desired, and this strength is governed largely by the ratio of the cement to the fine aggregate. The density test, then, has its value in the determination of the proper amount of coarse aggregate to use with a given mortar. This does not mean, of course, that the determination of mortar density is not of great value in obtaining the relative merits of two given sands, as it might develop in an analysis of this kind that one sand would work better in lean mixtures and poorer in rich mixtures, than another sand.

It is questionable whether any great value can be attached to results where a fixed percentage of water is used in all proportions from 1 : $1\frac{1}{2}$ to 1 : 3. The percentages of water absorbed

by the sand would be double in the extreme case and correspondingly large in the intermediate cases. The absorption would have to be determined and additions made for the mortar containing the larger amount of sand. It is also questionable whether a fixed percentage of water could be used, because of the known variation in amount of water required by different brands of cement to produce the same plasticity. It also seems hardly conceivable that 25 per cent of water will give anything but a very dry mortar, which would be practically impossible to mix with sand and compact thoroughly. It is common to use 24 per cent for neat briquettes, which are quite stiff, and in 1 : 3 sand mixtures it is not uncommon to use 12 per cent of the total weight or 48 per cent of the weight of the cement.

Mr. Kinney.

The making of briquettes of the material left from these density tests is hardly to be recommended. Variations equally as great as those shown could have resulted from the fact that one sample would have dried out more than another during the density test. New samples should be made up in every case. The argument sometimes raised against specifications requiring a mortar strength equal to that of Ottawa sand is not a good one, as the voids in Ottawa sand (a sand of practically uniform size) are 5 to 10 per cent greater than those of the ordinary sand. The ordinary sand, therefore, has a distinct advantage in percentage of voids and uniform grading. No good sand should fail to equal the strength of Ottawa sand in a 1 : 3 mixture.

The broad statement made by Mr. Moyer, that trap-rock screenings make a stronger and tougher concrete than the best sand, is an unfortunate one, inasmuch as this will be accepted by many as covering all kinds of screenings from all kinds of trap rock. While it is true that a great many stone screenings give excellent results in laboratory tests, there are few of them which produce good results in the field. This is due to the excessive amount of dust usually present in material of this kind, which has little or no effect under the best conditions existing in the laboratory, but is a serious detriment in the field. It is a safe precaution to require at least double the amount of mixing for stone screenings as for sand. It is better still to pass the screenings over a $\frac{1}{8}$ -in. screen and to use the coarser particles

Mr. Kinney. to add to the sand, as sand is usually deficient in the larger particles.

It is well in passing to say a word of caution in connection with the interpretation of tension tests on mortar. It has been quite conclusively proven that there is no direct relation between the strength of a given mortar in tension and its strength in compression. The ratio varies between wide limits, and inasmuch as concrete is used in compression almost exclusively, we should tend toward the compression rather than the tension test. For mortars, a 3 by 6-in. cylinder has been found convenient for use as a specimen and for concrete the 6 by 12-in. cylinder. While Mr. Moyer's paper brings out some new and interesting ideas, it is doubtful whether the method of proportioning suggested is a good one. The final proportions depend upon the void results of the coarse aggregates. This test, in my estimation, is a very inaccurate one, involving several variables. It seems to me far preferable to arbitrarily select a cement-sand ratio for a given sand of known value, and proportion the amount of coarse aggregate to give the maximum density with this particular mortar.

Mr. Thompson. **MR. SANFORD E. THOMPSON.**—There is a fundamental error in the principle brought out by the author of the paper, to which attention should be called: namely, the disregard of the effect of size of grain. The voids in sand are only one characteristic of the aggregate. The size of grain is of equal importance and to disregard it leads to erroneous results. In the methods suggested for testing mortar, the same amount of water is used in all tests, and this too may produce entirely erroneous results. A fine sand, as we all know, will require more water, and the density and the volume of a mortar is governed to a large degree by the amount of water used in mixing. Even when the water is varied so that the consistency is the same in two cases, a difference in size of grain, while not affecting the voids of the sand, may affect the density of the mortar to a large extent. The sands, one a coarse sand, with grains of uniform size, and one a fine sand, with uniform grains, may have the same percentage of voids and yet produce entirely different results.

In Mr. Moyer's tests of concrete there also is an omission of

an important factor, in that he fails to take into consideration **Mr. Thompson.** the sizes of the particles. His theory of concrete would be true if one could get the particles of sand and cement into the voids of the stone without separating the stone particles, but this is usually impossible. The grains of sand are graded from fine to coarse, and the fine particles of stone are practically the same size as the coarser grains of sand; consequently the sand particles do not fit into the voids between the stone particles and the bulk is increased. The method evolved is therefore incorrect from a practical standpoint.

MR. ALBERT MOYER.—Before there is any further discussion, **Mr. Moyer.** I want to correct what appears to be a misunderstanding. Nothing has been said in the paper as to the selection of aggregates; the paper is entirely on the subject of the proportioning of aggregates. It is presumed that the engineer has selected a proper and safe aggregate. That answer will meet some of the criticisms advanced by Mr. Chapman.

Mr. Kinney and Mr. Thompson have both assumed that I have stated that 25 per cent of water should be used in all these experiments with varying amounts of sand. I have made no such statement. I have simply stated that 25 per cent was used to determine how many pounds of Portland cement would make a cubic foot of paste. The amount of water is not stated as being 25 per cent. Except with mortars made with identically the same quantity of sand, the different proportions are obtained by varying the amount of paste.

GENL. W. H. BIXBY.—I notice that the author, in speaking **Genl. Bixby.** of the water test and measuring voids in sand, states that the sand, after being cooled, is dropped slowly into a graduated glass, jarring the glass to assist in expelling the air. I should think that the sand, dropped in that way, might, if jarred longer, possibly pack closer. I should like to ask Mr. Moyer whether any limit of time is put on the jarring in order to regulate the closeness of packing? Again, in determining the percentage of voids in crushed stone, the stone is thrown into the box loose, and then the top of the box is leveled off and the box is weighed. I should like to ask whether there is any jarring or shaking employed there to obtain close packing? Of course, on actual work in the field, we cannot take time to do all these

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Genl. Bixby. things; but for laboratory tests it seems essential or at least desirable to decide to what extent we are to have loose packing or close packing in getting our theoretical standards.

Mr. Moyer. MR. MOYER.—In answering General Bixby, I may say that I suppose I have tried further jarring many times and I find that I cannot get a sand to settle any further in the glass tube by continuing to jar the tube after as much air has been expelled as was expelled in putting the sand into the tube. As for placing the loose stone in the box and measuring it loose, the idea was to give a safe factor whereby the voids in the stone were actually more than might occur in the concrete, so there would be an excess of mortar which might overcome any unevenness in mixing. If the stone was shaken, the whole mass made as dense as possible and then the voids calculated, it might be a little too lean in actual work, due to unevenness in mixing.

Mr. Wig. MR. R. J. WIG.—There are several statements which might be criticised. Thus the author states that a 1 : 2 mixture is unnecessarily strong and that the 1 : 2½ mixture is sufficiently rich. These statements are ambiguous.

Again, he states "The comparison, therefore, should be made not on the basis of mortars of the same proportion, but on mortars of maximum density." I should like to ask the author how he would compare two sands, one of which would give 300 lb., say, mixed in the proportion of 1 cement to 2½ sand, and the other 275 lb. in a 1 : 3 mixture with two variables. How can one decide which is the better sand?

Furthermore, the weight of sand per unit of volume is not a constant as the author assumes, but is dependent upon the method employed in filling the measure and other factors.

Mr. Seaman. MR. H. B. SEAMAN (*by letter*).—The danger of laboratory studies is in their tendency to over-refinement and to impracticable results as applied to field work of great magnitude.

It is generally accepted that the maximum density of the aggregate will produce the greatest strength in the concrete—other things being equal. For this reason we are using "run-of-crusher" in broken stone, but in some cases, I think, we are making a mistake in eliminating the smaller sizes.

It is impracticable to use the theoretically exact amount of cement needed. We use an excess just as we adopt a factor of

safety. Practical mixing is always imperfect, and the theoretical amount of water is variable. Mr. Moyer places the water at 25 per cent; others will say 22 per cent. Yet either of these percentages will produce concrete many times stronger than needed in general work. Such variations in the work are covered by the factor of safety. Mr. Seaman.

The best specifications for proportioning concrete which have come to my notice, are those of Mr. Gustav Lindenthal for the Connecting Railroad Bridge, New York City, namely: "One volume of cement shall be taken to mean 380 lb. net. One volume of broken stone or gravel shall be taken to mean $3\frac{1}{2}$ cu. ft., packed or shaken down." With these specifications a 1 : 2 : 4 proportion with compacted material will approximate 1 : $2\frac{1}{2}$: 5 proportion of loose aggregate. But loose aggregate varies greatly according to its dampness, or other temporary conditions, while a compacted aggregate, particularly if compacted by puddling, will be as nearly uniform as can be obtained.

In making laboratory tests it is important to bear in mind constantly its practicable application to field work; and to make this application as simple as possible.

MR. MOYER (*Author's closure, by letter*).—In answer to Mr. Chapman, I wish to state that I do not advocate the tension test as part of a specification. My paper is not a suggestion as to a specification for the selection of sand or other aggregates. The paper is carefully worded and it covers only the subject of the proportioning of previously selected aggregates. The tension test mentioned in the paper is specifically stated to be only a check against various other tests which might indicate whether the proportions used produce maximum density. The compression test is, of course, considerably better as a check or an indication as to the density of the mass. Unfortunately, few laboratories are equipped with compression machines; therefore this test was not included in the paper. Mr. Moyer.

In reply to Mr. Kinney, it would appear that his discussion is largely made up of statements or opinions not backed by a thorough investigation by means of experiments or tests. It appears to be a criticism directed against my paper as an attempt on my part to standardize experiments which, together with other experiments, will enable any one to better determine

Mr. Moyer. what proportions with any given aggregate will produce maximum density.

In the glass-tube experiment which has been carried on successfully by Westinghouse, Church, Kerr & Co., under Mr. Chapman, for a number of years, I advocate using 25 per cent of water. This has been stated to be incorrect. This percentage of water is used with an amount of cement to produce a standard consistency of paste. The slight overfilling of voids with the cement paste is one of the fundamental principles emphasized in the paper. In this experiment, the quantity and quality of the sand used remain fixed, therefore the consistency of the paste used to fill the voids in order to standardize this experiment should remain fixed, and the only way it can remain fixed is to state the percentage of water. This has nothing whatever to do with the selection of the sand. It is an experiment which is not intended to indicate the value of the sand for concreting purposes. It is simply one of the experiments which will help to determine the correct proportion of cement paste to the available sand that is to be used to produce a mortar which is a little over-cemented so that the mortar can properly bind together the larger aggregates. It has been found by experiment that 25 per cent of water is amply sufficient for the leaner mixtures. It is therefore more than sufficient for the richer mixtures. The mortar is sufficiently dry for compact tamping. More accurate results can be obtained by tamping such mortars in a glass tube than by puddling or any other means.

I beg to differ with Mr. Kinney as to his statement that it is common to use 24 per cent of water with neat cement in making up standard briquettes and that this percentage of water produces a rather dry mixture. The percentage of water required for Lehigh Valley cements of established reputation is from 20 to 22 per cent.

In regard to Mr. Kinney's criticism as to the use of trap-rock screenings, which he states is an unfortunate choice inasmuch as it may be accepted as covering all kinds of screenings and all kinds of trap rock, I think I can safely state that no kind of trap rock will produce poor screenings. There may be too much dust in the screenings, but this can be easily taken

care of. Trap rocks are such stones as green stone, basalt, **Mr. Moyer.** trachyte, amygdaloid. All these rocks are melted rocks which are pushed up through the crust of the earth and cooled gradually. The very nature of their formation causes them to be preferred for concrete as they all break, both naturally and artificially, in spheres or cubes, and are not of a laminated structure.

In answer to Mr. Thompson's statement that there is a fundamental error in the principal part of my paper in that the effect of size of grain is disregarded so that the results are erroneous, I may say that the results from the glass-tube test are values actually obtained. No theory is needed in that connection. In so far as the theory of size of grain is concerned, I have been aware of it for many years but I have yet to find any reason for attempting to apply it. Properly proportioned concrete, proportioned by the methods described in this paper, when sawed through and polished, does not show enough minute holes, even when examined under a glass, to warrant any regard whatever to the theory as to the presence of some grains of sand too large to fill the voids produced by the others.

The various methods described in this paper do and have produced concretes of maximum density under many varying conditions; consequently these methods may be regarded as correct from a practical standpoint.

BLAST-FURNACE SLAG AS AGGREGATE IN CONCRETE.

By W. A. AIKEN.

SUMMARY.

The object of the investigation, the results of which are given in the paper, was to determine the value of blast-furnace slag as aggregate in concrete. Preliminary tests were first undertaken to develop the relative value of this material compared with limestone and Birdsboro trap rock, in order to determine whether a more extended investigation of furnace slag was warranted. As a result of this preliminary work, five hundred 6-in. concrete cubes were made up in the proportions of 1-part cement, 2 parts sand, and 4 parts slag, by volume.

A standard Lehigh Valley Portland cement, normal in every respect, and Jersey gravel, material almost universally used as sand in the vicinity of Philadelphia, and crushed slag, commercially called $\frac{3}{4}$ -in. material, were used throughout this investigation. The concrete was mixed by hand to ordinary work consistency, rather wet than dry. One hundred test specimens were broken at the earlier periods, 28 days, 3 months and 6 months, respectively, but the number was reduced to 50 each at 9 months and 1 year. The remaining hundred specimens were kept for later periods, it being proposed to break ten each at 6-month intervals up to 6 years.

Compressive tests furnished the following average data:

AGE.	COMPRESSIVE STRENGTH, LB. PER SQ. IN.
28 days.....	1561
3 months.....	1952
6 months.....	2589
9 months.....	2841
1 year.....	2797

These results are differentiated and discussed in detail in the paper.

From the actual strength of the concrete; the weight of the slag per cu. ft. (which is less than that of most material used similarly); the recognized solubility of slag which permits it to act as a puzzolanic material; its alkaline nature, which is especially conducive to rust-prevention in reinforced-concrete construction; and from the relative high combined percentages of silica, alumina and iron, making for permanency in the resulting concrete; we conclude that slag of similar constitution is in every way satisfactory for use as aggregate in concrete.

BLAST-FURNACE SLAG AS AGGREGATE IN CONCRETE.

BY W. A. AIKEN.

Every new or extended utilization of by-product makes for economy in the conservation of resources. The millions of tons of blast-furnace slag annually produced in the United States, for years considered not only practically worthless, but always a nuisance, and entailing unavoidable expense in handling and disposal, have from time to time to some extent been utilized in a variety of ways; one very important use of certain of these slags being in the manufacture of a valuable product, Portland cement. But the percentage of furnace slag made use of thus, or indeed in any other way, such as for track ballast or for building macadam roads, is at present relatively very small, and yet is actually considerable in tonnage. Any increase in the use of this product to the extent outlined in this paper, should the adaptability of slag for concrete aggregate be generally recognized, would furnish a very desirable outlet. To be sure, its adaptability for this purpose has always been recognized by large iron manufacturers, who early made use of this handy material when concrete construction was undertaken around their plants; but it has not been adopted generally.

With the marvelous growth in recent years of concrete construction, and especially reinforced concrete, the advantage of furnace slag as aggregate in reducing the dead weight per cubic foot of concrete, has made a strong appeal to many concrete engineers. The result is that the building codes of several of our large municipalities have permitted the use of furnace slag equally with any other material ordinarily used for aggregate in concrete.

In the course of our professional work during the past year, we have made an extended investigation of certain slag-concrete. The particular value of our work we think lies especially in the fact that any conclusions to be drawn therefrom, are based on a large number of test results, very much

more than the number from which averages are ordinarily deduced. In consequence, we feel that any deductions submitted are reliable; and while tests on slag-concrete specimens have often been made previously and the results in some cases published, so far as our examinations of these are concerned the number of tests from which average results were deduced at each period were comparatively limited, and consequently the conclusions therefrom are correspondingly less reliable.

Originally, we made a few comparative tests using different aggregates, such as limestone, Birdsboro trap rock, and slag; the tests being carried on only to the 28-day period. This was done to arrive independently at the relative actual values of these different materials. As a result of these early tests and the showing made therein by slag, we undertook an extended series of tests involving the manufacture of five hundred 6-in. cubes; 100 of these to be crushed at each of the several periods, 28 days, 3 months, 6 months, 9 months and 1 year, so as to arrive at entirely reliable averages, and to establish the uniformity of the character of the concrete. As the work progressed, results were such that the number of cubes tested at the 9-month and 1-year periods was reduced to 50 each, and it is proposed to crush the remaining 100 cubes at 6-month intervals up to 6 years—10 cubes at each period.

The materials used in this investigation were all commercially procured in Philadelphia, and the work of making the specimens was no better than that under ordinary field conditions of concrete construction—no attempt having been made to enter into any elaboration. Thorough mixing, however, was positively assured, the work being done by hand. The molds used were in gangs of tens and made of planed 1-in. white-pine lumber, put together with screws.

The cement used was a standard Lehigh Valley brand showing the following compliance with the standard specifications of the American Society for Testing Materials:

Fineness.—No. 100 sieve, 96.0 per cent passing; No. 200 sieve, 81.2 per cent passing.

Time of Set.—Initial set, 5 hr. 30 min.; final set, 6 hr. 10 min.

Constancy of Volume.—Passed the cold-water-pat, air-pat, steam-pat, and boiling-pat tests favorably.

Tensile Strength.—(Average of 5 briquettes.)

	NEAT.	
24 hours.	7 days.	28 days.
219 lb.	601 lb.	704 lb.

1 CEMENT: 3 SAND.

7 days.	28 days.
272 lb.	347 lb.

Temperature of laboratory air, 70° F.

Temperature of gaging water, 68° F.

The sand used was Jersey gravel, the material almost universally used in the vicinity of Philadelphia. This showed the following granulometric analysis:

SIEVES.	AMOUNT PASSING, PER CENT.
¾-in.....	97.4
No. 10.....	90.8
No. 20.....	73.8
No. 30.....	63.8
No. 40.....	55.2
No. 60.....	35.4
No. 100.....	11.6

This is not ideal material, as will be recognized, but was used because it is the material of the market.

The coarse aggregates, commercially called ¾-in. material, all passed the 1½-in. sieve and were retained on the ½-in. sieve.

The slag used showed the following chemical analysis:

Silica (SiO ₂).....	34.40 per cent
Alumina and Iron Oxide (R ₂ O ₃).....	23.40 "
Lime (CaO).....	35.88 "
Magnesia (MgO).....	3.21 "
Sulfuric Anhydride (SO ₃).....	0.39 "
Sulfur (S).....	1.27 "
Loss on Ignition.....	1.34 "

All material was measured by volume in the proportions of 1 part cement, 2 parts sand and 4 parts coarse aggregate; this being the requirement of the Philadelphia Building Laws. All concrete was mixed to ordinary work consistency—rather wet than dry.

The preliminary investigation for comparative purposes gave the following average results from three 6-in. cubes at 7 days and three 6-in. cubes at 28 days:

Coarse Aggregate.	Compressive Strength, lb. per sq. in.	
	7 days.	28 days.
Limestone.....	390	941
Birdsboro Trap Rock.....	1144	1888
Slag.....	1681	2537

There was then undertaken the main investigation covering the manufacturing and testing of the 500 slag cubes. These were all air stored in a dry cellar, being sprinkled with water once a week. They gave on breaking the following results:

COMPRESSION TESTS ON 6-IN. SLAG-CONCRETE CUBES.

AVERAGE WEIGHT OF THE CONCRETE BEING 140.8 LB. PER CU. FT.

Time at which Tested.	No. of Specimens.	Average Compressive Strength, lb. per sq. in.	Remarks.
28 days....	100	1561	73 per cent of all the tests averaged 1533 lb. per sq. in.; within 2 per cent of the general average, but lower. 20 per cent of all the tests averaged 1730 lb. per sq. in.; within 11 per cent of the general average, but higher. 7 per cent of all the tests averaged 1344 lb. per sq. in.; within 14 per cent of the general average, but lower.
3 months..	100	1952	78 per cent of all the tests averaged 1922 lb. per sq. in.; within 2 per cent of the general average, but lower. 15 per cent of all the tests averaged 2185 lb. per sq. in.; within 12 per cent of the general average, but higher. 7 per cent of all the tests averaged 1794 lb. per sq. in.; within 8 per cent of the general average, but lower.
6 months..	100	2589	73 per cent of all the tests averaged 2583 lb. per sq. in.; practically in complete agreement with the general average. 14 per cent of all the tests averaged 3058 lb. per sq. in.; within 18 per cent of the general average, but higher. 13 per cent of all the tests averaged 2125 lb. per sq. in.; within 18 per cent of the general average, but lower.
9 months..	50	2841	72 per cent of all the tests averaged 2874 lb. per sq. in.; within 1 per cent of the general average, but higher. 8 per cent of all the tests averaged 3367 lb. per sq. in.; within 18 per cent of the general average, but higher. 20 per cent of all the tests averaged 2514 lb. per sq. in.; within 12 per cent of the general average, but lower.
1 year.....	50	2797	84 per cent of all the tests averaged 2812 lb. per sq. in.; within 1 per cent of the general average, but higher. 6 per cent of all the tests averaged 3534 lb. per sq. in.; within 26 per cent of the general average, but higher. 10 per cent of all the tests averaged 2342 lb. per sq. in.; within 16 per cent of the general average, but lower.

A study of this detailed differentiation of test results shows that at 28 days, 3 months and 6 months, of the number of individual tests failing to closely agree with the several general averages, the large percentage of these show considerably greater strength than these general averages or than the average of the larger percentage of tests which agree so closely with the several general averages. The average strength noted in all the tests at these periods, 28 days, 3 months and 6 months, may therefore be assumed as thoroughly conservative.

A similar study of results at 9 months and 1 year shows that of the results not in close agreement with each period's general average, or with the larger number of tests at each period which closely approximate these general averages, a somewhat larger percentage falls below this general average than that which runs above. But as those results which are above the general findings average relatively much more *above* such general findings, than the low results average *below* these general findings, it may again be assumed that the average strength results of all the tests at the later periods of 9 months and 1 year are equally conservative.

This is more particularly evident when it is borne in mind that the sand used was not what could be considered first-class material and undoubtedly influenced the strength of the concrete developed. Also, the comparatively small size of the slag aggregate must necessarily have influenced the strength of the concrete, while the results would probably have been higher, had the test specimens been larger. It is also to be noted that in so far as the author's observation goes, the results are markedly lower than those published by other investigators of slag concrete.

The main point is whether the compressive-strength values herein developed are sufficiently great to warrant the employment of slag as aggregate, in competition with broken stone and gravel. We think the findings are in favor of this, since our experience has been that the crushing strength of broken-stone or gravel concrete, made up under ordinary field conditions, will not average over 1500 lb. per sq. in. at the age of 30 days.

Consequently, from the actual strength of the concrete

developed in these tests; its weight per cubic foot (which is less than that of most materials used similarly); the recognized solubility of slag, which permits it to act as a puzzolanic material; its alkaline nature, which is especially conducive to rust-prevention in the case of reinforced-concrete construction; and from the relatively high combined percentages of silica, alumina and iron, which make for permanency of the resulting concrete: we conclude that slag of similar constitution is in every way satisfactory for use as aggregate in concrete.

DISCUSSION

Mr. Walter. MR. L. W. WALTER.—The paper presented by Mr. Aiken is of peculiar interest, in that it deals with a subject on which comparatively little has heretofore been written, and furnishes reliable data which will be of material aid in determining the suitability of a substance, economic in many localities, as a bulky constituent of an important building material.

When we consider the widely-known physical characteristics of air-cooled blast-furnace slag, and the economic advantages which its use in concrete would afford in many localities, it is quite natural that we should make inquiry as to the reasons why its use as a concrete aggregate has been so limited in the past, and as to whether these constitute sufficient reasons why it should not be more generally used as such in future concrete construction.

I desire, therefore, to present for your consideration two reasons why the use of slag as a material in concrete has been so limited, and endeavor thereby to encourage discussion of a subject which must be dealt with in the concrete industry of the future.

1. Among these reasons must be recognized a lack of confidence in slag as a concrete aggregate, due rather to a limited individual experience with its use and to a very limited publication of data by those who have used it, than to specific objection to it based on personal experience with it, or on a knowledge of its behavior where used by others.

2. There exists a quite prevalent idea that slag contains elements which may act detrimentally when in combination with cement.

Let us see to what extent these are factors. They are certainly not factors as affecting construction and maintenance of masonry at the large iron and steel plants, where slag has for several years been used to the exclusion of other coarse aggregate in concrete. Here the use of slag was first limited to structures, failure in which would not be vital to the operation of the plants,

but confidence once established seems to have begat confidence, Mr. Walter. and as a result there may be seen at most steel plants, slag concrete in all classes of masonry, including foundations subjected to the pounding of heavy machinery, the stoppage of which, but for an hour, might entail a loss compared with which the first cost of the foundation would be insignificant. I know of no higher compliment that can be paid to slag as a concrete aggregate than its continued use in mill construction by those who have used it most, and who are in position to know it best. This, however, should not be accepted as an argument in favor of its use in concrete for all purposes other than mill construction.

Let us make inquiry as to what is known of slag as a concrete aggregate by those who have not been associated with iron and steel mill construction. Ask of the average man interested in the concrete industry, "What do you know of slag as an aggregate in concrete?"; "Have you ever used it?"; "What do you think of it?" Then figure on a percentage basis and you will likely conclude that the limited use of slag in concrete for general purposes is due in part to a lack of knowledge as to its behavior where used, and to a suspicion rather than to a knowledge on the part of some that slag as an aggregate will encourage disintegration in concrete.

Each has its bearing on past usage of slag. Each, therefore, will have its influence on future usage until there is sufficient data available to enlighten those interested as to what has been done. In order to allay all fears, these data must be of such a nature as will justify confidence based on both test results and a knowledge of the behavior of slag concrete in service for several years in the various kinds of structures and classes of work.

The revenue to be had from the utilization of slag as a waste by-product, and the desire for a material of economic advantage, should stimulate the publication of reliable data concerning the use of slag in concrete, but the commercial feature must not be the predominating factor, as slag must stand on its merits. Its adaptability for use in concrete road construction, in typical highway and railroad structures, and for general building purposes, must be judged from experience with its use, separately in the different classes of work.

Mr. Walter.

The question as to whether the puzzolanic properties of slag add to its value as a coarse aggregate in concrete seems of less importance than does the question as to whether the elements in slag cement, which act detrimentally to concrete, will also act detrimentally when contained in the slag used as a coarse aggregate. If it could be proven that the elements in slag, which give rise to doubt and which, if active at all, are active only when the slag is in a finely pulverized state and remain inert in coarse aggregate, a determination as to the suitability of air-cooled bank slag for use in mass concrete could be made, apart from matters theoretical, and confidence in slag as an inert material could be more easily established. Its alkaline characteristics would seem to be a point in its favor for use in both plain and reinforced concrete.

My desire to encourage a healthy discussion of the subject presented by Mr. Aiken is prompted by experience based on an investigation, covering a period of several months, of all data obtainable, as well as on a close examination of slag concrete at steel plants, in aggregate quantity amounting to several hundred thousand cubic yards used in general open-hearth and rolling-mill construction, and in no less than twenty structures elsewhere, including piers, abutments, arches, culverts, retaining walls, flat-top beam and rail spans, and involving foundations, bridge floors, railing and wall coping.

As regards test results published, it is unfortunate that few are comparable, one group with another, since both the size and shape of the test specimens as well as the proportioning of the aggregates have varied in different tests. For example, Mr. Aiken reports a strength in 6-in. cubes, of 1 : 2 : 4 concrete, of 1561 and 1952 lb. per sq. in. at 28 days and 3 months, respectively. In a recent test, in which the writer was interested, 8 by 16-in. cylinders of 1 : 2½ : 5 concrete averaged 1266 and 1446 lb. per sq. in. at corresponding periods, using different cement, different sand and different slag.

It is reasonable to suppose that the difference in strength shown as between the two series of tests is due in part to the different lots of cement and sand used and to the difference in size and shape of the test specimens. It would be unreasonable to suppose anything other than that the difference is due in

part to the different proportions of the constituent materials. Mr. Walter. It seems therefore that no one series of tests, unless it be very comprehensive in scope, can furnish results which will in themselves justify final conclusions, when the interest at stake is of so much concern as that of determining the suitability of a material for unlimited use as a concrete aggregate.

The sum total of all comparative test results which I have been able to collect indicates that concrete in which slag was used compares favorably in strength with that in which either limestone, trap rock, or gravel was used as a coarse aggregate. Based on a careful examination of concrete in actual structure service, I am inclined to the opinion that the failures or partial failures in slag concrete have not been proportionally greater than in concrete in which other materials of accepted good quality have been used.

MR. J. S. UNGER.—Mr. Aiken's paper on the subject Mr. Unger. of a comparatively new material to be used as an aggregate in concrete is presented at a time when the attention of most engineers is drawn to the subject of concrete aggregates.

Within a decade concrete has largely supplanted stone and brick in foundations, walls, arches, dams and similar structures. New uses for concrete are appearing daily. As it exists, in what may be called a plastic state, in the early stage of manufacture, it is readily formed to almost any shape that may be desired.

Considerable prejudice to the use of slag aggregates has existed in the past, and to some extent at the present time, in the minds of those who may have been asked to use a blast-furnace slag aggregate in concrete. In nearly every case they have not been able to understand why a product which has always been considered a waste, and of no value whatever, could have valuable properties. They have confused blast-furnace slag, properly prepared, with all wastes of a similar nature, such as cinders, ashes, various melting furnace cinders, clinkers and improperly prepared blast-furnace slag. Slag is not as universally obtainable as gravel or broken stone, being confined to iron manufacturing localities, but where the cost of transportation is not excessive, it is a substitute, which for practical purposes is the equal, if not the superior, of the aggregates commonly used.

Mr. Unger. Blast-furnace slag must be properly prepared. If the molten slag is suddenly cooled by water, it breaks up into a sand or light material like pumice stone. If cooled by being cast in a cold mold, it cracks and breaks up into small pieces, having very little strength. It must be cooled slowly to have the greatest strength. The method adopted in our district is to pour the molten slag on a slag dump in layers about 6 in. thick, then allow it to cool in the air. As the layer of slag is hot underneath, the cooling on a large slag dump, covering two to three acres, is moderately slow, as a considerable quantity of the heat lost is made up by frequent fresh additions of molten slag. When the dump has been filled, the slag is left to cool and weather. This permits any shrinking, cracking, or disintegration to take place, and allows it to weather, in order that any soluble matter may be leached out. This weathering period is usually a year or more. We believe that the slag should remain exposed for six months at least before being dug up by a steam shovel, and afterwards dried, crushed and screened for the market.

The slag produced in the Pittsburgh district, where most blast-furnaces are run on Lake ores, is of a fairly constant composition, presenting no greater variables than would be found in gravel, broken limestone, or broken rock. The final crushing will break up into small particles or dust any soft portions of slag, which will be eliminated by screening, leaving only the hard slag as a material for aggregates.

Mr. Aiken presents a table covering 400 specimens. The small size of the specimens, 6-in. cubes, and the richness of the mixture, 1 : 2 : 4, may be used by some as an argument against his results, but I believe the results are truly comparative within themselves, in showing what influence time has on the setting qualities. We made a number of tests using cylinders 12 in. in diameter, having about three times the area of those used by Mr. Aiken, and a mixture of 1 : 3 : 6, comparing these cylinders with similar ones made in exactly the same way, using other aggregates. The results of these tests indicated that the slag aggregate was the equal of any of the other aggregates used.¹

¹ A full account of these tests is contained in a publication which may be obtained from the Carnegie Steel Co.

Our practical experience with slag aggregates began in a **Mr. Unger.** small way about ten years ago. During the last five years, we have used it extensively for all kinds of concrete work. At some plants it is now used exclusively. Our engineers are a unit in saying that from their own observations it shows fewer through cracks than other aggregates, produces a smoother surface, is less liable to chip or spall on weathering, and that the erection of a structure may be commenced on slag concrete sooner than on other concrete; the reason being that the mortar takes a stronger grip on the aggregate on account of its irregular shape and porosity. Where gravel is used and a pebble is close to the surface, or exposed, the concrete soon weathers and the pebble becomes loose and falls out. Slag aggregate acts differently. The irregular shape and the slight pores, which have been covered with a coating of mortar having little roots, grip the aggregate and hold it fast.

Slaking of slag has been charged against it, but when this happens, an investigation will usually prove that the slag used is not a blast-furnace slag, or if so, that it has been improperly prepared and weathered. Some blast-furnace slag undergoes a form of disintegration or granulation on the dump when cooling. This is usually termed slaking, but is not true slaking as might be applied to a piece of freshly burned lime. This change takes place in from 24 hours to 1 week after being exposed. As this powder is practically gotten rid of in the crushing of the slag, it can be neglected.

The high sulfur content does not seem to affect the steel reinforcement. In a case with which I am familiar, two 8-in. beams were anchored in slag concrete for 2 years, and when removed the ends were as good as when placed. Another case is the removal of an old foundation after being in service 7 years. The foundation bolts were not corroded in the least, having the same appearance as when put into service. Director Knaff read a paper before the Society of German Iron Manufacturers in which he calls attention to a foundation bolt originally embedded in liquid slag, containing 1.29 per cent of sulfur, which showed no trace of rust when removed after 19 years. He also refers to another case in which a beam was buried in a slag-lime mortar for 14 years. The slag carried 1.67 per cent of

Mr. Unger.

sulfur. When the beam was removed, no corrosion was shown, the beam was still covered with the original roll scale and carried the red-lead markings of its manufacture.

The weight of blast-furnace slag aggregate is an important consideration where the weight is an object, as in floor or roof construction. When compared with gravel, it weighs about 2000 lb. per cu. yd., while average gravel weighs about 2800 lb., or 40 per cent more.

The use of blast-furnace slag aggregate will nearly always be confined to such localities close to its point of manufacture, but as its valuable properties become better known, it will largely supplant all aggregates with which it can compete in cost.

Mr. Humphrey.

MR. RICHARD L. HUMPHREY.—I have listened with considerable interest to the discussion on the use of slag concrete and the use of blast-furnace slag in concrete. I have had occasion to act as expert in cases where trouble has resulted from its use, and I believe that a few precautionary words concerning blast-furnace slag may be useful.

I cannot quite agree with Mr. Unger that blast-furnace slag is as easy to obtain as crushed stone. Considerably more care has to be taken in the preparation of blast-furnace slag for use in concrete than in the preparation of crushed stone.

Before discussing blast-furnace slag as an aggregate of concrete, I wish to state that if Mr. Unger will investigate the numerous comparative tests that have been made of stone and gravel concrete, and the numerous structures in which gravel concrete has been used, he will find that his statement that the gravel weathers out of concrete is not justified. Gravel is among the best aggregates available for use in concrete.

Blast-furnace slag has a wide range from the acid to the basic. Basic slag weathers, and is gradually disintegrated. I am not willing to accept the statement of Mr. Unger that if slag which has partly disintegrated is crushed, there will be no further weathering, for the reason that basic slag that will weather in the slag pile, will continue to weather wherever it is exposed in the structure in which it is used, the weathering consisting in a slaking of the lime; and if this occurs in a structure of concrete, it will result in its disintegration. If the slag is embedded

so as to be protected from the weather, the slaking is not so likely to occur. Mr. Humphrey.

If blast-furnace slag, containing sulfur, is used in a very dense concrete, there is not likely to be a corrosion of embedded metal. If, however, the concrete is porous, permitting the moisture and air to come in contact with the sulfur and the metal, corrosion will take place. The sulfur usually occurs in slag in the form of sulfides which oxidize on exposure to air, and cause disintegration. Sulfur, instead of not being objectionable, is a dangerous element in slag that is used as an aggregate of concrete.

The range in the density of slag from the very heavy, dense acid slag, such as may be obtained in the old slag piles at Hockendauqua and Catasaqua, to the very porous basic kind that is usually obtained from blast furnaces, represents the difference between a suitable and a thoroughly unsuitable slag. The dense slags above referred to were those made of acid slag which were poured into iron pots and allowed to cool slowly, just as Mr. Unger has described. This material, when broken up, affords a very dense excellent material which cannot be excelled for use in concrete. On the other hand, from this same district comes porous slag which is wholly unsuitable. Most of the blast-furnace slag of the present day is basic, and unsuitable as an aggregate of concrete. The objection to the use of porous slag, except where a very light filling material is required, as in floors, and which is not used as a structural material, is that the cement and fine aggregate go into the pores and result in a considerable shrinkage in the volume of the resulting concrete. Care must be taken in using such an aggregate that the sand is placed on the slag first, and then the cement; otherwise the cement if used first is likely to go into the pores and therefore decrease the strength of the resulting concrete.

There is a great deal of excellent slag available for use as an aggregate of concrete, that is tough and dense, and fully as good as crushed stone. A proper blast-furnace slag for use as an aggregate of concrete should be acid, dense, tough, and free from sulfur. Such slag has the appearance of stone. Slag prepared as Mr. Unger has described, makes an excellent aggregate for concrete, and there is a field for it. Before using a blast-furnace

Mr. Humphrey. slag as an aggregate of concrete, however, an investigation should be made to determine whether the slag is suitable. An unsuitable slag is one of the worst aggregates that can be used.

Mr. Chamberlain. MR. O. P. CHAMBERLAIN.—While I did not see the paper under discussion until this morning, one point which attracts my attention is the apparently high silica content in the slag used in the concrete referred to. I assume that the strength of the slag used in these tests is dependent to a great extent upon its high silica content. While the source of supply of the other ingredients of the aggregate is mentioned, nothing is stated in this paper as to where this slag was secured. Offhand, it seems to me that the silica content is extraordinarily high.

The slag used in these experiments contained as stated 34.4 per cent of silica, which I judge is an unusually hard slag.

I wish also to emphasize one point brought out by Mr. Humphrey—that tests made of a single kind of slag do not necessarily mean that cement-slag concretes in general are satisfactory. There are slags that are good for concrete work, and there are slags that are very bad.

Mr. Hasse. MR. O. C. HASSE.—I just want to relate an experience I had about three years ago in the use of slag concrete. This concrete had been placed in the fall and the following spring the contractor found that he had neglected putting in some foundation bolts, so he drilled holes for these bolts and found that the concrete had attained a blue-green color; he became alarmed and subsequent investigation showed that a fresh crushed slag had been used in the make-up of this concrete. This crushed slag contained 1.74 per cent of sulfur, which was responsible for coloring the concrete. Compression tests made on this concrete gave fairly good results at that time, yet grave fears were entertained that when the sulfur oxidized to sulfuric acid it would weaken the concrete or tend to disrupt it altogether. It was therefore taken up and replaced with gravel concrete. If the sulfur content of the slag had been low enough, no doubt this coloring of the concrete would not have occurred, and thus proved satisfactory. This serves to indicate the necessity of exercising great care in the use of crushed slag for concrete purposes; it is quite a variable substance, as we all know.

MR. W. A. AIKEN.—I wish to say that from the tests we Mr. Aiken. have made, we conclude that slag of similar constitution will be satisfactory. There are poor slags and good slags, of course. This slag happened to be a good slag, as the tests and analysis show. We would, of course, not endorse all slags without investigation.

(*Author's closure, by letter.*) Mr. Humphrey's statement that "gravel is among the best aggregates available for use in concrete" needs qualification just as much as he has instituted, when this discussion emphasizes that there is a vast difference in the characteristics and consequently in the values of slags. In the author's very extensive experience with gravels, this material is often of more than questionable value; for instance, water-worn gravel of shaley origin is liable to grind up in the concrete mixer with very unsatisfactory results, so that the fact that there are poor slags unfit for use as aggregates should no more condemn this material than the fact that there are poor gravels should condemn the use of gravel; nor is it important that emphasis should be put on such patent differences in material.

The value of the particular material reported on in this paper is distinctly emphasized on account of test results therewith and certain chemical characteristics. One of these, the high silica content, is distinctly referred to by Mr. Chamberlain in his remarks. No broad claim can properly be made for a material, the chemical composition of which varies as widely as that of slags, but given a material of a certain character, and a sufficiently large number of test results to establish averages for a properly prepared product obtainable of approximately the same character over wide sectional areas in many manufacturing centers, conclusions certainly can properly be drawn as stated. This is apparent, too, from Mr. Walter's reference to other slags, for which he gives no data as to chemical composition, but which, as stated by him and known to every one who has had broad experience, have stood up satisfactorily under service conditions quite as severe as those to which any concrete made of other than slag aggregates may have been subjected.

TESTING CONCRETE AGGREGATES.

BY CLOYD M. CHAPMAN.

SUMMARY.

In this paper is proposed a method for testing the coarse aggregate to be used in concrete. The method consists in subjecting to known compressive stress specimens built up of the coarse aggregate, which has previously been coated with a cement grout. The voids between the pieces of aggregate are filled with a standardized grout, and after aging the specimen is tested in compression. Results of tests made in the laboratories of Westinghouse, Church, Kerr & Co. by the method described are given.

TESTING CONCRETE AGGREGATES.

BY CLOYD M. CHAPMAN.

So much has been said and so little progress made in the matter of testing concrete aggregates, that the concrete industry finds itself at the present time in a peculiar position. It is using vast quantities of materials, which are agreed upon to be extremely variable and therefore very much in need of testing, and yet it is without any recognized standard method of testing or of interpreting results of tests.

Many concrete engineers appreciate the necessity of testing the sand and stone as well as the cement to be used in concrete. There are fairly definitely prescribed standard methods for the testing of cement, but the testing of the sand and stone is left largely to the individual preference of the engineer. The result is that while many engineers test these materials, they do so according to their own ideas. As a result, there are now in use about the same number of methods of testing sand and stone as there are laboratories engaged in the work.

In the matter of testing, sand seems to have received a great deal more attention than the coarse aggregates. This may be, and probably is, due to the fact that more troubles with concrete have been charged against the fine aggregate than against the coarse aggregate. This is perhaps quite true, but there is no doubt that the strength of a concrete is due in a considerable measure to the coarse material used. Mixed with the same cement and sand in the same proportions, some stones will give much stronger concrete than others. For the strongest reinforced work it is important that a quality of stone be used that will make the best concrete.

Experiments made in the laboratory of Westinghouse, Church, Kerr & Co. indicate that one stone may produce concrete from 50 to 100 per cent stronger with the same proportions of the same sand and cement than another stone.

If both of these stones were available for the same job,

and if the work at hand were one requiring concrete of maximum compressive strength, there is no question as to which ought to be used. There should, however, be some standard method adopted for determining the relative value of the available materials for any important piece of concrete work.

It is not an easy matter to decide just what tests shall be applied to stone. Hardness, density, weight per cubic foot, and percentage of voids may in some cases have a bearing on quality, but they do not serve to enable one to choose the stone which will make the strongest concrete. For instance, the strongest concrete made in the series of tests above referred to was made from a light porous material, having a comparatively low compressive strength and a high percentage of voids. The making up of concrete compression test specimens of the cement, sand and stone in varying proportions is a very commendable practice, but the results are functions of so many variables that it is quite impossible to determine from them the specific effect of the coarse aggregate used on the strength of the specimen.

In an effort made in the laboratory of Westinghouse, Church, Kerr & Co. to develop some more useful form of test for coarse aggregates, there was tried, among others, the method proposed in this paper, which seems worthy of further investigation and development by those most interested.

The author's method in its present form consists in determining the compressive strength of a specimen made up of what may be called a skeleton structure of the material under investigation, the interstices of which structure have been filled in with a standardized neat cement or a cement-sand grout, in a sufficiently fluid state to flow into and fill all the voids.

Fig. 1 shows such a skeleton structure, made of a well-rounded gravel, before the grout has been poured in. The pieces of stone are in close contact with one another, and if held in place by a strong binder to prevent the slipping of one stone upon another, the total compressive strength of the skeleton framework might be readily determined. It is the purpose of the standard grout to furnish the binder to hold the stones in place for crushing.

The resulting compressive strength is a function of many variables, yet probably of a less number than if the stone and

grout were mixed together to make a concrete before being placed in the mold.

The details of manipulation are as follows: The molds used are 6-in. cylinders, 6 in. high, and 2-in. cubes. The size of stone used is $\frac{3}{4}$ to $1\frac{1}{4}$ in. (averaging about 1 in.) for the 6-in. specimens, and $\frac{1}{4}$ to $\frac{1}{2}$ in. for the 2-in. specimens. Sufficient stone to fill the mold is soaked in water for 24 hours to saturate it so that water will not be absorbed from the grout.

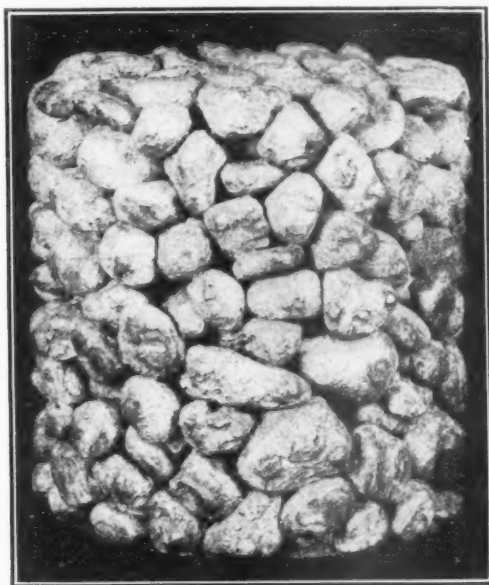


FIG. 1.—Aggregate Test Specimen before adding Grout.

Sufficient grout is made up of a mixture of equal parts by weight of Portland cement and a good grade of sand, screened through a 20-mesh sieve, with about 35 per cent of water added, for the 6-in. cylindrical specimens, and of neat cement with about 45 per cent of water added for the 2-in. cubes.

The wet stones are mixed in with the grout until they are thoroughly coated. The mixed stone and grout are poured on a $\frac{1}{4}$ -in. sieve and the surplus grout allowed to drain off. The

stones, coated with grout, are placed in the mold and are packed down well, shaken while being put in place, so that they are in close contact with one another. Fig. 1 shows a 6-in. specimen at this stage of the process.

The remainder of the grout is well stirred up and poured

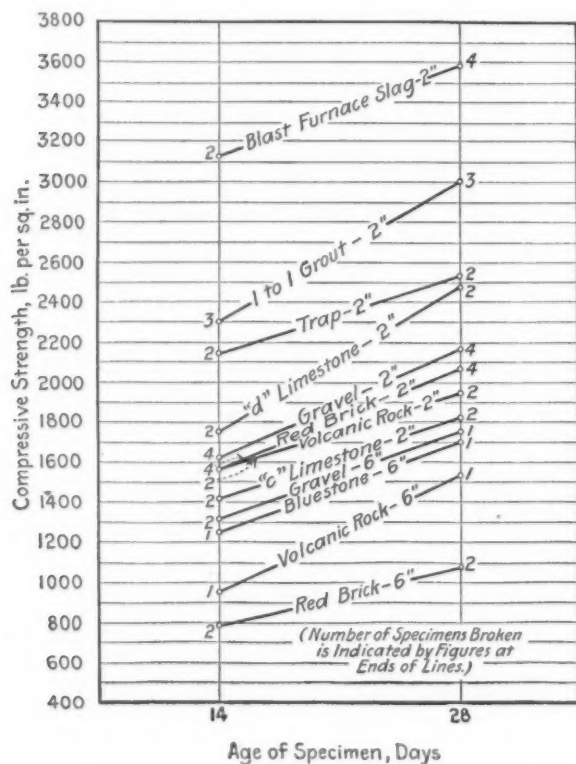


FIG. 2.—Compressive Strength of 1:2:5 Concrete in the Form of 2-in. Cubes and 6-in. Cylinders.

into the mold near one side, jarring the mold continually so that all the voids between stones will be filled. The top of the specimen is finished smooth by adding grout and by slight troweling after the cement has begun to harden a little.

The 6-in. specimens are stored in a damp closet until broken at the ages of 14 and 28 days, while the 2-in. cubes are stored in

water. The specimens are faced with a thin layer of plaster of Paris a few hours before crushing.

The results obtained are shown graphically in Fig. 2.

It may be noted that the several stones tested ranged over quite wide limits. The points plotted represent averages of from 1 to 4 specimens.

It is greatly regretted that there are not many more specimens of the 6-in. size. There are others under way, but at the time of writing the specimens have not been broken.

It is hoped that this method, together with any others which are considered promising, may be thoroughly investigated by a properly constituted committee of the Society, and by individuals and institutions sufficiently interested, in order that we may have at no very distant date a standard method of testing these materials.

DISCUSSION.

Mr. Johnson.

MR. N. C. JOHNSON (*by letter*).—Mr. Chapman's paper on "Testing Concrete Aggregates" was passed without discussion at the session of the meeting at Atlantic City at which it was presented, because, in all probability, others were unprepared, as was the writer, to make specific comment at that time. Any method of testing aggregates that promises rational results is of too great importance to be passed by without due consideration; and it is hoped that the following presentation of the subject will be of service in determining the worth of this test.

To say that the strength of concrete depends primarily upon the strength of the materials entering into it, seems almost unnecessary. There is no escape from a statement of fact so essentially basic. It is doubtful, however, if we are customarily giving this obvious fact its due importance, or remembering it in selecting materials for our every-day concrete.

It is also trite and almost unnecessary to say that the concrete which in density most nearly approaches homogeneous natural stone of proper strength and character is the best concrete for any given requirements. For instance, if a certain load is to be carried, concrete of a density equal to that of a natural stone, the normal strength of which would be capable of withstanding the given stress, would be the most economical concrete to use, a proper factor of safety being assumed.

We determine the strength of a natural stone by stressing to the breaking point a piece of the stone of definite cross-sectional area. It is easily determined that the strength per unit area is a certain amount, say 18,000 lb. per sq. in. The determination of this unit strength is simple, for we are dealing with a homogeneous material. Further, this unit strength is inherent in and evenly distributed throughout the mass, that is, the strength of the cube expresses the total cohesive strength of all the tiny particles which, ages ago, united to form the rock mass, of which the cube was a part.

It must, therefore, be true that the fragments of the broken

cube possess inherently the same strength per unit area as Mr. Johnson. the original mass; and following the reasoning a step further, if it were possible to replace these fragments in their original relation one to the other and to hold them in this relation by some cementitious material, each layer of this material being infinitely thin, and of a strength equal to the adhesion or cohesion between the tiny particles that formed the stone, we could impose a load on the re-formed cube equal to that carried by the original cube. If such a replacement and cementing were possible, we should have an ideal concrete.

Indeed, some stones are of themselves natural and ideal

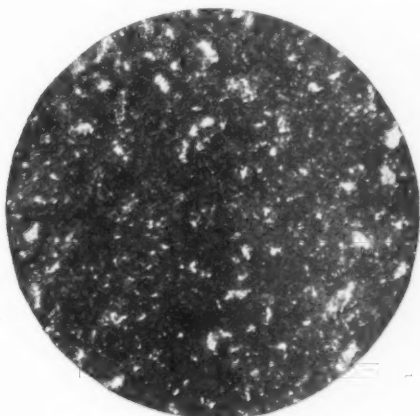


FIG. 1.—Soft Sandstone. ($\times 300$.)

concretes. If we magnify a pebble sufficiently, we render visible its component grains; and it is possible that we may also see the cementing layer between these grains, though as we do not always know what to look for, we may not recognize it. The accompanying microphotographs illustrate such natural concretes. Fig. 1 shows a sandstone, almost so soft as to be shaly. A high magnification was required to bring out this structure. Fig. 2 shows a micaceous sandstone wherein the component aggregates—tiny, but none the less true aggregates—and their boundaries, with possibly also the cementitious material between them, may be more easily distinguished. These are true concretes in miniature and a tantalizing ideal.

Mr. Johnson.

In practice, however, the ideal cannot be reached. We may have all the fragments of the crushed cube but neither their perfect rearrangement nor the infinitely thin and strong cementitious layer can be had. We are limited in both respects, but to overcome the first limitation we add to the fragments cementitious material diluted with sand.

But these conditions in no wise change the inherent strength of our stone fragments. The true measure of their unit strength remains the unit strength of the mass from which they were broken; and if properly bedded they are capable of exerting this strength in concrete. To determine their actual strength

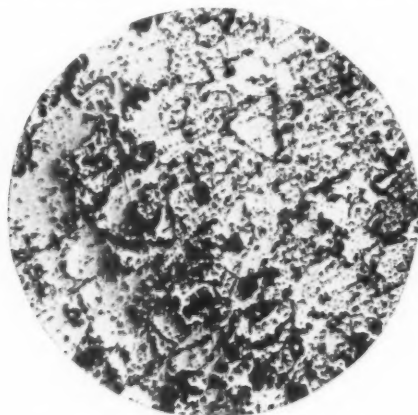


FIG. 2.—Micaceous Sandstone. ($\times 60$.)

in such a substance, it remains, then, to pack them together in a relation similar to that which they would hold in a commercial every-day concrete; to keep them in that relation; and to bed them thoroughly in a matrix *having a strength at least equal to the inherent strength of the fragments*. In such an arrangement, the crushing strength of the mass must closely approximate the inherent strength of the stone.

Mr. Chapman's method of aggregate test would seem to offer a nearer approach to the ideal than any in present use. There are, however, some considerations which seem to have been overlooked.

The first is a question as to the obtaining of a matrix suffi-

ciently strong to allow the full working stress of the stone to be developed. For the sake of eliminating as many variables as possible, let us assume that neat cement grout is used to fill the voids in the stone, as sand would introduce new and indeterminate strengths in the matrix. And in this connection, further difficulties may be encountered, as a neat cement grout sufficiently thin to enter the voids might shrink unduly. But assuming the end desired to be accomplished with a normal paste and all voids filled, we are confronted with the fact that unless the crushing strength of the stone is less than 14,000 lb. per sq. in., which is a generous figure for the crushing strength of normal, neat cement of reasonable age, the test specimen will fail by crushing the matrix, rather than by crushing the stone. And corollary to this is the second consideration that if the matrix crushes, the adjacent fragments, or pieces of gravel, having point contact, may crush below their actual strength, as stress values at contacts would momentarily rise extremely high, even with low unit loads on the specimen.

Illustrative of this point, the compressive strengths of stones of various kinds shown in Table I are interesting, as compared with the figure before taken as that of the compressive strength of neat cement, that is, 14,000 lb. per sq. in.

TABLE I.—COMPRESSIVE STRENGTH OF STONE.

Reference.	Kind of Stone.	Average Compressive Strength, lb. per sq. in.			
		2-in. Cube.		8-in. Cube.	
		Edge.	Bed	Edge.	Bed.
1	Gneiss (Chester, Pa.).....	6 097	5 446	9 505	6 426
	Gneiss (Germantown, Pa.).....	19 891	15 555	11 636	13 984
	Granite (French Creek, Pa.).....	19 997	14 348	17 274	7 910
	Mica schist (Conshohocken, Pa.).....	20 038	15 680	10 417	7 532
	Sandstone (Curwensville, Pa.).....	10 218	8 013	7 513	4 463
	Quartzite (Lumberville, Pa.).....	14 841	8 637
2	Limestone.....	13 000
	Marble.....
3	Sound trap rock, 4-in. cubes.....	33,300 lb. per sq. in.			
	Seamy trap rock, " ".....	19,400 " " " "			

¹ R. L. Humphrey—*Engineering and Mining Journal*, June 20, 1902.

² Gen. Q. A. Gillmore.

³ Watertown Arsenal tests.

Mr. Johnson.

From these results, it would seem that Mr. Chapman's method might give erroneous results with the great majority of aggregates commonly used, as the strength of the matrix would probably be below that of the aggregate under test.

It would be interesting also to compare the results as given on Mr. Chapman's curves with the values of the coarse aggregate in bulk. A tentative comparison of trap rock as aggregate and in bulk may be made from Mr. Chapman's curve and from the figures before cited. As aggregate, the trap rock shows a strength at 28 days of about 2500 lbs. per sq. in. In bulk, the trap rock tested at the Watertown Arsenal had a maximum strength of 33,300 lb. per sq. in. and a minimum of 19,400. It would seem as if these figures tend to confirm the correctness of the reasoning that indicates the test as being one for matrix, rather than for aggregate.

The varying strengths with different aggregates shown on Mr. Chapman's curves may result from greater or less voids, with consequent thicker or thinner matrix layers between the fragments of aggregate. It would be both interesting and instructive to illustrate these points further with other photomicrographs that the writer has in his possession, but a more detailed presentation of this subject is beyond the province of the present discussion.

It would seem that with slight modification, Mr. Chapman's method could be made to give very accurate results; and it is needless to say that some standard method of testing large aggregate is badly needed. If, instead of neat cement, or a cement grout of ungraded sand as a filler between the pieces of large aggregate, a standard grout of strong, carefully graded sand, preferably crushed quartz, or crushed trap were used, in quantity equal to voids plus a definite percentage, a matrix of definite thickness of layer and of strength equal to the inherent strength of the aggregate would be secured. With such a matrix, properly aged, concordant results closely approximating the true strength of the aggregate should be obtained.

Mr. Chapman.

MR. CLOYD M. CHAPMAN (*Author's closure, by letter*).—The practical value of any laboratory test of the materials of construction is in direct proportion to the degree of accuracy with which the test indicates those qualities of the material which it

must possess in order properly to perform its functions in the finished structure. **Mr. Chapman.**

Without going into a discussion in detail of Mr. Johnson's comments in the matter of testing concrete aggregates, it seems desirable to point out and emphasize the salient points which must be kept prominently in mind when considering any test for materials of this character. One of these is that "ideal" conditions are not to be sought. They are not desired. They would produce, if attained, only "ideal" results. What are wanted are purely practical conditions in order that we may secure practical results.

Another point to be remembered is, that the problem is to produce a method of test which will determine which of two or more aggregates will produce the strongest concrete—not which aggregate is in itself the hardest, or densest, or has the greatest compressive strength, or excels in some other quality. The one thing to be determined is the quality of concrete it will produce, and the only way to do so is to test it in the form of concrete in some manner or other. To do this satisfactorily there should be but one variable entering into the test and that variable should be the aggregate itself.

In the test as developed in the laboratory of Westinghouse, Church, Kerr & Co., there has been an effort made to eliminate as many variables as possible. The grout used is of course "standardized" as mentioned in the paper. The sand used for the grout in the tests reported was carefully sized by screening.

The test offered may not prove equal to the demands for materials of this nature. The way to find out is to try it. Improvements can undoubtedly be made, and perhaps some entirely different methods will be developed, but neither improvements or new methods will be developed with the pen or on the convention-hall floor; they must be worked out in the laboratory or in the field.

It is greatly to be hoped that, with so many vitally interested in the problem, there will be much done in the near future in the way of investigation and try-out.

RELATION BETWEEN DEFORMATION AND DEFLECTION IN REINFORCED-CONCRETE BEAMS.

By G. A. MANEY.

SUMMARY.

In this paper the author attempts to show that a very intimate relation exists between the deformations at the two extreme fibers of any reinforced-concrete beam and the deflection. Some difficulty has been experienced in the past in attempting to check the deflection formulas commonly used with actual measured deflection. This has probably been due to the number and indefinite nature of the empirical constants involved, although some authorities have been inclined to question the accuracy of the experimental work.

A comparatively simple formula is here deduced which makes the deflection a function of only one quantity which for any given condition must be estimated. This quantity is the deformation at the most-stressed section in the extreme fibers. The formula is based upon the fact that the moment of the area under the M/EI curve, when properly taken, is a measure of deflection. Then by expressing M/EI in terms of the deformation at the extreme fibers, an expression for the deflection of a reinforced-concrete beam is determined which makes it a function of these deformations entirely.

Very satisfactory experimental verification of this formula has been found. The results seem to show that the deformation at the most-stressed section only need be considered, and that influence of stirrups and bent-up bars is practically negligible. It is also shown that the effect of tension in the concrete on decreasing the deflection at low loads consists only in reducing the deformation at the most extreme fibers, upon which the deflection is made to depend for its value.

It is thought that this formula will be a practical one for the designer because of its comparative simplicity, and that its accuracy will be limited only by the accuracy with which the maximum deformations can be estimated.

RELATION BETWEEN DEFORMATION AND DEFLECTION IN REINFORCED- CONCRETE BEAMS.

BY G. A. MANEY.

An examination of the deflection formulas for reinforced-concrete beams now in use would seem to indicate that a state of considerable uncertainty exists with regard to the particular features of the action of reinforced-concrete beams which are the determining factors in their deflection. The use of these formulas becomes somewhat difficult because of the number of constants involved and because of the indefinite nature and value of some of these constants.

The attempt will be made in this paper to show that the unit deformations in the steel, and in the extreme fiber of the concrete, are the only determining factors in the deflection, except the depth, span and method of load-distribution.

A formula is here derived and well verified by reliable tests, by which the deflection is equal to the product of a constant into the sum of the deformations at the two extreme fibers. In any given case this constant depends for its value only on the dimensions of the beam and the method of loading. The formula is based upon a certain property of the M/EI curve for any beam, namely, that the deflection of the beam is proportional to the statical moment of the area under the M/EI curve. This curve is obtained by dividing each ordinate of the bending-moment curve by the product of the corresponding values of E and I , which are the modulus of elasticity of the material and the moment of inertia of the sectional area of the beam, respectively.

As a practical method of finding deflections, this property applies to homogeneous beams only. A precise statement of the property as here used, with certain modifications for reinforced concrete, follows.

The statical moment of the area under the M/EI curve between any two points on the elastic curve, taken about one

of these points, is equal to the vertical deflection of this point from a tangent to the elastic curve at the other point. (The beam is assumed to be horizontal.) This may be proved as follows:

Proof.—The unit stress on the extreme fiber is equal to Mc/I . The unit deformation, e , of that fiber is therefore equal to Mc/EI , and the total deformation in the distance dS along the elastic curve equals $McdS/EI$. Referring to Fig. 1, it is seen that this total deformation of the extreme fiber, divided

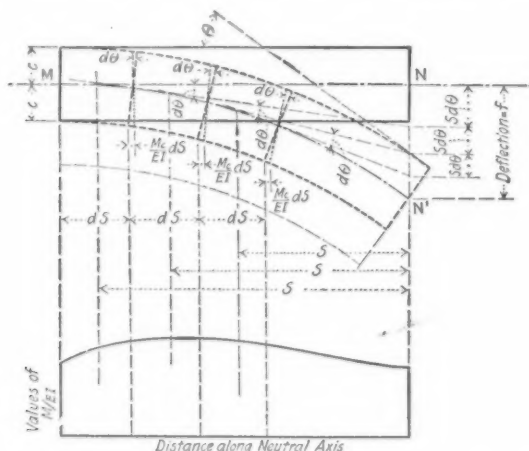


FIG. 1.— M/EI Curve and Section along Beam before and after Bending.

by the distance, c , from that fiber to the neutral axis, is equal to the change of slope, $d\theta$, in the distance dS . Or

$$d\theta = \frac{Mc}{EI} \frac{dS}{c} = \frac{M}{EI} dS \dots \dots \dots (1)$$

It will be seen that the term $M dS/EI$ is equal to the area under the M/EI curve for the distance dS along it; that is, $d\theta$ is equal to a differential element of the area under that curve.

Now, let S equal the distance from any differential element dS to the point at which the deflection is desired. Then, referring to Fig. 1, it is clear that the deflection of N' from the tangent MN at M is equal to the sum of the products of each $S d\theta$

along the elastic curve from M to N , S being measured from N' . Or

$$f = \sum S d\theta \dots \dots \dots (2)$$

But $S d\theta$ is equal to the statical moment of the differential element of the area under the M/EI curve about N' , by the definition of statical moment. Therefore, the deflection of any point from the tangent to the elastic curve at any other point, is equal to the statical moment of the area under the M/EI curve about the first point. *Q. E. D.*

(In this proof, as in all treatments of the elastic theory, it is assumed that the projection of the neutral axis after bending, upon its original position, is the same as its length before bending.)

This property may be conveniently expressed in the following form for homogeneous beams:

$$f = kl^2(M/EI) \dots \dots \dots (3)$$

The value for M/EI in equation (3) will be taken for the point in the beam at which the bending moment is maximum. Then the value of k is such that the statical moment of the M/EI area between any two points, which is equal to the deflection of one point with respect to the tangent to the elastic curve at the other, is expressed in terms of this maximum value of M/EI and the span of the beam, l .

It is evident that the value of k for any case is equal to the coefficient of deflection for homogeneous beams in the well-known equation $f = k_1 W l^3/EI$, divided by the coefficient for the maximum bending moment, which in general may be expressed as $M = k_2 Wl$, from which $W = M/k_2 l$. Substituting this in the last equation for deflection, we have:

$$f = k_1 \cdot \frac{M}{k_2 l} \cdot \frac{l^3}{EI} = \frac{k_1}{k_2} l^2 (M/EI) \dots \dots \dots (4)$$

Comparing equation (4) with equation (3), it is seen that $k = k_1/k_2$. To illustrate, consider the deflection at the center of a freely supported beam with a single concentrated load at the center; that is, the deflection of the support with respect

to the tangent to the elastic curve at the center. The coefficient k_1 is $1/48$, and k_2 is $1/4$. Therefore, the coefficient k in equation (3) will become $1/12$ ($=0.0833$; see Fig. 3 (b)).

The maximum deflection of a simple beam is equal to the deflection of the support from the *horizontal* tangent to the elastic curve. Usually the maximum deflection occurs at the center of the beam, as in all cases of symmetrical loading. With irregular loading, it usually occurs near the center of the beam. Then, by the property of the M/EI curve above referred to, we may express this deflection as the statical moment about the support of the area under the M/EI curve between the point at or near the center of the span, where the slope is zero, and the support. If this statical moment is expressed in terms of the maximum ordinate of the M/EI curve and the whole span, an expression is obtained of the form of equation (3), in which the coefficient k will equal k_1/k_2 .

It may be of interest here to point out that M/EI equals the rate of change of the slope of the elastic curve, or the change of slope per unit distance. This may be seen by dividing equation (1) by dS , which gives $d\theta/dS = M/EI$.

DEFLECTION OF REINFORCED-CONCRETE BEAMS.

The application of this principle to the deflection of reinforced-concrete beams consists in finding a convenient expression for the value of M/EI .

Let e_c and c_c be the unit deformation in the extreme fiber and the distance from the extreme fiber to the neutral axis, respectively, for the concrete, and let e_s and c_s be the same quantities for the steel. In a homogeneous beam, as above indicated, $e/c = M/EI = d\theta$, and is the same for both extreme fibers. In this case, since the values of EI are constant throughout the length of the beam, e/c varies directly as M throughout the span.

In a reinforced-concrete beam (see Fig. 2),

$$\frac{M}{EI} = d\theta = \frac{e_c}{c_c} = \frac{e_s}{c_s} = \frac{e_c + e_s}{d} \dots \dots \dots (5)$$

Substituting this value of M/EI in equation (3), the following expression for the deflection of a reinforced-concrete beam is obtained:

$$f = k \frac{l^2}{d} (e_c + e_s) \dots \dots \dots (6)$$

This is based on the usual assumption that a plane before bending remains a plane after bending, which seems justifiable from the evidence of reliable tests herein referred to.

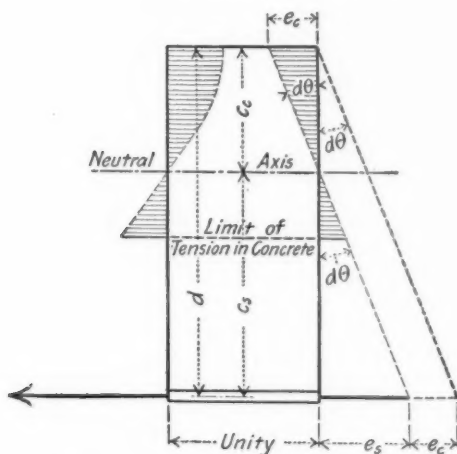


FIG. 2.— $d\theta$ and the Variation of Stress and Deformation over the Beam Section.

From the preceding analysis it is evident that the deformations of the extreme fibers are the only determining factors in the deflection, except the span, depth of beam, and load-distribution—these latter all being known for any given conditions. It is also evident that the distribution of the stresses in the steel and concrete over the section have no influence on the deflection, except in so far as they influence the deformations of the extreme fibers.

The influence of tension in the concrete might well be discussed here. We know from the principles of equilibrium (see Fig. 2) that the effect of the tension in the concrete at low

stresses is to reduce to some extent the compressive stresses in the concrete and the tensile stresses in the steel. This means that we might expect a stiffer beam in the earlier stages of the loading, as the steeper slope of deflection and deformation curves show.

For the reason, therefore, that tension exists in the concrete and that usually near the end of the beam the rods are

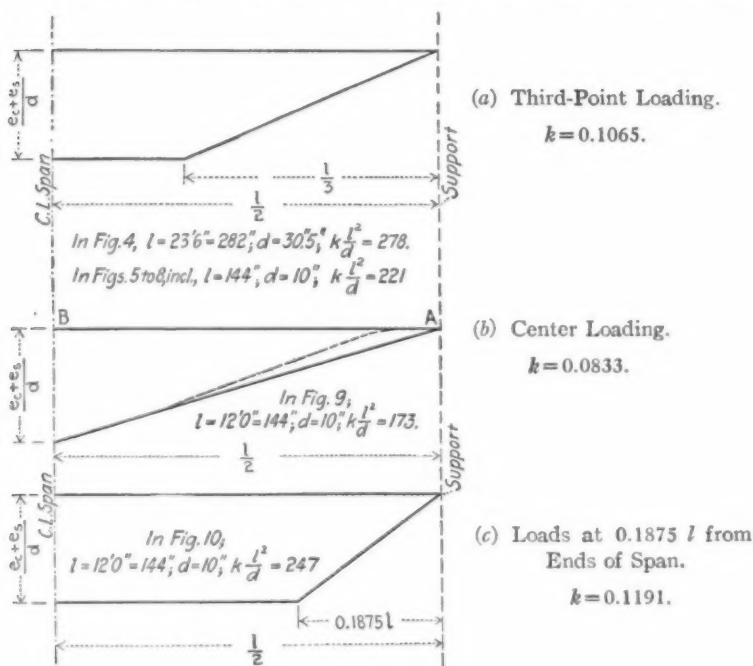


FIG. 3.—Variation of $\frac{e_c + e_s}{d}$ Values between Supports and C.L. Span.

bent up, we know that the value of the $(e_c + e_s)/d$ curve, with which we deal to obtain the deflection, will not have the same variation as the values of M .

Near the supports of a simple beam and at all points where the bending moment is small, we would expect considerably smaller values of $(e_c + e_s)/d$ than the value of M at such a point, relative to the value of M at the point of maximum moment, would indicate. The values of $(e_c + e_s)/d$ would probably

follow the dotted line indicated in Fig. 3(b) because of the tension in the concrete. A glance at Fig. 3(b) will show, and computations will prove, that a small decrease in values of $(e_c + e_s)/d$ ($= d\theta$) near the point about which moments are taken (the support, in this case), changes the value of this moment only slightly.

In all the curves shown in Figs. 4 to 14, inclusive, which represent tests on beams made by the Engineering Experiment

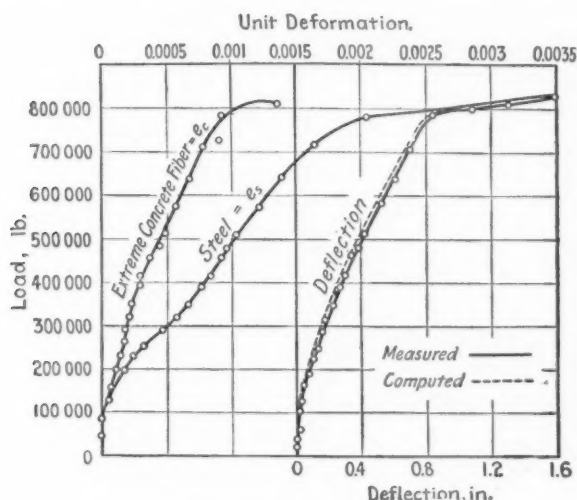


FIG. 4.—Deformation and Deflection Curves of a Reinforced-Concrete Beam. Tested by A. N. Talbot. 1 : 2 : 5 Concrete; 1.26 per cent steel (square corrugated bars); Third-Point Loading.

Stations of the Universities of Illinois and Wisconsin, deformations in the concrete and the steel were measured at the point of maximum bending moment only, which is at the middle for center loading and between loads where they symmetrically applied. When the sum of the extreme fiber deformations in the concrete and the steel at this point of maximum deformation were multiplied by the constant indicated in Fig. 3 for their respective conditions of span, depth and load-distribution, a deflection curve which is indicated by a dotted line in each

of Figs. 4 to 14 is obtained, agreeing very closely in every case with the actual measured deflection curve.

This fact indicates that it is proper to make the deflection a function of the deformation in the extreme fibers at the most-stressed section, and that the effect of tension in the concrete and of the change in the moment of inertia due to bent-up bars is negligible. Where such bars are used they are always bent up at points where the bending moment is comparatively small, and the decrease in the moment of inertia of a section due to the bent-up bars would be counterbalanced to some degree by the increase of the moment of inertia due to the larger percentage of tensile stress in the concrete at this section.

Figs. 4 and 5 to 10, inclusive (Plate I), give curves of e_c , e_s , and deflection for some beams tested by Mr. A. N. Talbot.¹ They cover widely varying percentages of steel and three different methods of application of load. The areas, between the center of the span and the support, of which moments about the support are taken for these three different cases of loading, are indicated in Fig. 3.

When moment-area is expressed in terms of $(e_c + e_s)/d$ and l , we get the constant k , indicated. The uniformly close agreement of the dotted line which is plotted according to the formula, and the deflections actually measured, is interesting.

Figs. 11 to 14, inclusive, show the results of tests made by Mr. M. O. Withey,² in 1907. These are all T-beams and have either stirrups or bent-up rods.

Tests of three large beams are described by Mr. A. N. Talbot.³ These beams are 23 ft. 6 in. long, 6 ft. 3 in. wide, 30½ in. in effective depth, and have 1.26 per cent of reinforcement. Many of the rods are bent up, and stirrups are used outside the third points. These beams are larger than most beams used in practice and the details are typical. Deformations in the concrete and the steel were taken on 50-in. gage lengths at the middle, and the beams were loaded at the third points.

The results for one of these beams are shown in Fig. 4.

¹ Bulletin No. 4, University of Illinois Engineering Experiment Station.

² Bulletin No. 197, University of Wisconsin Engineering Experiment Station.

³ Bulletin No. 28, University of Illinois Engineering Experiment Station.

PLATE I.
 PROC. AM. SOC. TEST. MATS.
 VOL. XIV, PART II.
 MANEY ON REINFORCED-CONCRETE BEAMS.

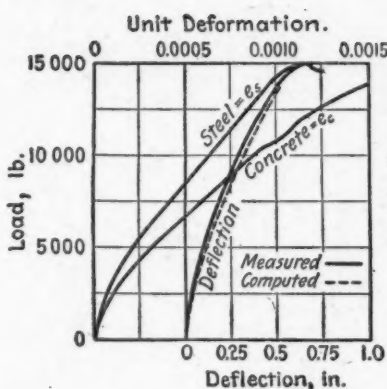


FIG. 5.—1.84% Mild Steel.

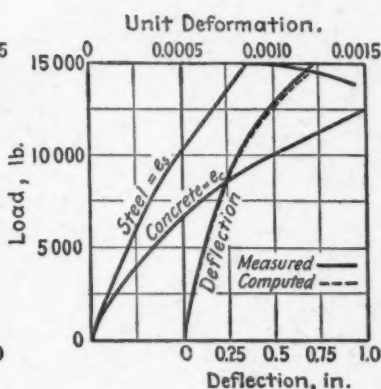


FIG. 6.—2.76% Mild Steel.

THIRD-POINT LOADING.

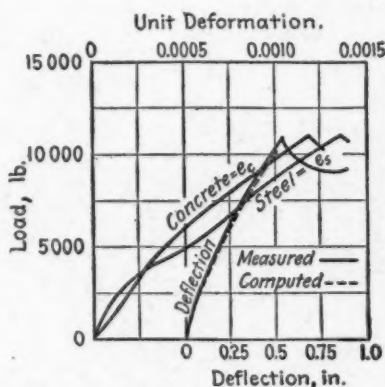


FIG. 7.—0.98% Mild Steel.

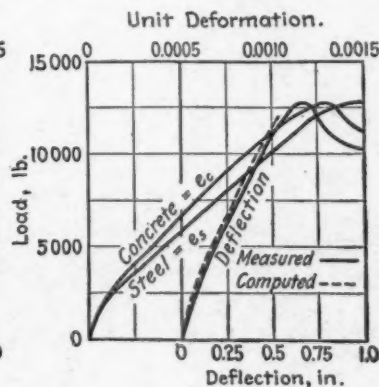


FIG. 8.—1.24% Mild Steel.

THIRD-POINT LOADING.

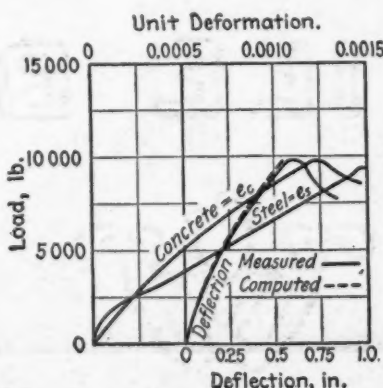


FIG. 9.—0.98% Mild Steel.

CENTER LOADING.

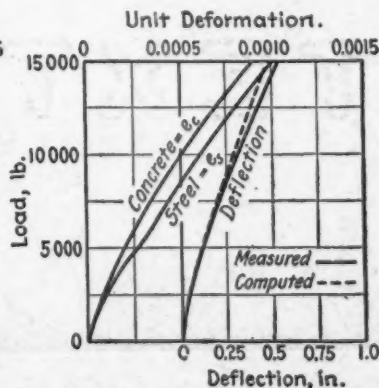


FIG. 10.—0.98% Mild Steel.

LOADS AT 0.1875 l FROM ENDS OF SPAN.

DEFORMATION AND DEFLECTION CURVES OF REINFORCED-CONCRETE BEAMS. TESTED BY A. N. TALBOT.

1:3:6 Concrete; Percentage of Steel and Loading as Indicated.

(See also Fig. 3.)

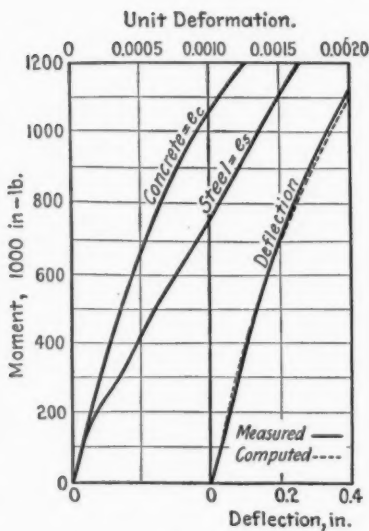


FIG. 11.

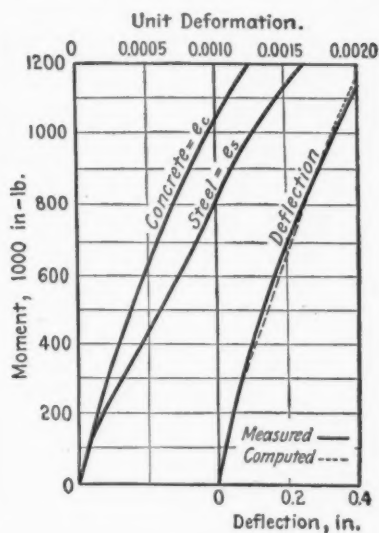


FIG. 12.

BARS BENT UP.

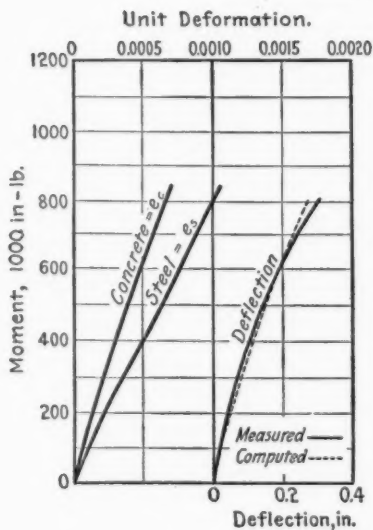


FIG. 13.

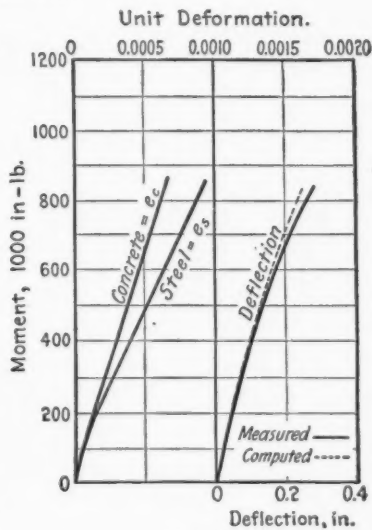


FIG. 14.

BARS STRAIGHT.

DEFORMATION AND DEFLECTION CURVES OF REINFORCED-CONCRETE T-BEAMS. TESTED BY M. O. WITHEY.

1:2:4 Concrete; 1.04 per cent Steel; Third-Point Loading.

For this beam as for the other two, when the deformations measured in the concrete and in the steel are used to obtain the deflections by substituting them in the formula proposed in this paper, a deflection curve is obtained as shown by the dotted line, which agrees very closely at all loads with the actual measured deflection.

Values of k for maximum deflections under several conditions of loading are here given:

Beam with uniform load:

Ends freely supported, $k = 5/48$ or 0.1041;

Ends fixed, $k = 1/32$ or 0.0313.

Beam loaded at the third points:

Ends freely supported, $k = 23/216$ or 0.1065;

Ends fixed, $k = 5/144$ or 0.0347.

Beam loaded at the middle:

Ends freely supported, $k = 1/12$ or 0.0833;

Ends fixed, $k = 1/24$ or 0.0416.

EXAMINATION OF CONCRETE FAILURES FOR THEIR DETERMINING CAUSES.

BY R. S. GREENMAN.

SUMMARY.

The reasons for poor concrete have been proportioned as being 90 per cent due to poor workmanship, 8 per cent due to poor aggregates, and 2 per cent to poor cement. These reasons and the additional one of the influence of the water used, are responsible for most failures in concrete. These failures are more frequent in smaller than in larger work and the resulting losses are proportionately greater.

To find a reason one must examine all possible causes and must follow up all clues that tend toward disclosing the reason. The design, the quality and proportions of the materials used, the mixing and placing, and the care of the finished concrete must all be considered. In making an examination, each case must be treated entirely upon its own individuality; and the right clue will not be found unless the investigator knows good and poor concrete and knows why certain things produce certain results in concrete.

To illustrate the possibilities of various influences, specific cases are cited showing effects of certain elements upon water, sand and stone and the resulting effects upon the concrete. The effects of poor workmanship are seen in various conditions found in concrete. To find these effects it is also necessary that the investigator shall know how to grasp and to follow up the first indications of there having been anything wrong.

Some external influences also cause concrete to fail and the possible relation of these to concrete failures must be considered.

To aid the examination, proper use should be made of laboratory tests and analyses, and between the work in field and laboratory some logical reason for the failure should be secured.

EXAMINATION OF CONCRETE FAILURES FOR THEIR DETERMINING CAUSES

BY R. S. GREENMAN.

Concrete is said to be its own best inspector, and it is a well-known fact that defects in concrete will sooner or later make their presence known.

For every fault there must be a reason. The reasons for poor concrete have been proportioned as being 90 per cent due to poor workmanship, 8 per cent due to poor aggregates, and 2 per cent to poor cement. These percentages are not the result of tabulations but are those prevailing in the minds of many who have had considerable opportunity for inspection of concrete, both good and bad. But whether or not these percentages are correct, the statement raises the question, "What are the reasons for poor concrete and how do we determine these reasons?" It is a certainty that neither a laboratory test nor a field inspection alone will give them, unless, of course, it be a simple failure. Yet there are people who will examine a piece of concrete in the laboratory and offer a solution of the problem simply by tests made there. And again, an inspector will look over a piece of concrete construction and with no knowledge whatever of the characteristics of the materials used, nor of the method of making, will attempt to tell how such and such a condition developed. Concrete failures can only be explained after thorough investigation by men who know good or bad concrete from long and close acquaintance, and whose minds are of an analytical and judicial temperament.

The more one sees of concrete the more one becomes convinced that it is the most abused structural material being used. The science of making concrete has been looked upon too generally as being very easily learned, with the natural result that a great deal of poor concrete has been made. Now, however, builders, contractors and engineers apparently desire to make concrete that will meet the standard in quality required of other materials, and yet, with all the precautions now being taken,

there is still a large amount of concrete that is not satisfactory. The larger the work the greater the care taken; and, vice versa, the smaller the work the greater the carelessness and the greater is the ratio of the failures, and it is the sum total of these failures that makes the loss caused by poor concrete so great.

In trying to solve a problem of poor concrete the elements to be investigated are the three already mentioned—cement, aggregates and workmanship—and water. The common tendency is to first place the blame upon the cement, but if we find that in accordance with good and generally common practice, the cement has been tested and has met the standard requirements, the cement then becomes a negligible factor; but if it has not been tested, it must be considered as a possible cause, and it may become a large item in the study. We must admit that since so much stress has been laid upon the value of having cement tested before use, and since it must indeed be a small work where it has not been so tested, the percentage of failures due to poor cement has been reduced to a very small amount.

If the next element has been given equally as thoughtful consideration previous to its use, there could not be so much poor concrete due to poor aggregates. The strong and weak points in both the coarse and fine aggregates have been too often neglected. The coarse aggregates can usually be judged by easy inspections, but sand or other fine aggregates need very careful examination. Its characteristics—such as the grain, the grading, the cleanness, and its freedom from organic impurities and excess of loam—are items of knowledge which are obtainable mainly in the laboratory, but which are very essential for the correct diagnosis of a concrete failure.

The effect of workmanship is by far the largest factor, and in it all others are included, for a poor workman can destroy the value of the best materials. Under the head of workmanship must be considered such items as design, proportions, placing, and actions resulting from heat, frost, electrolysis, etc., which should have been taken care of during the process of making, hardening and preservation.

As another element of importance, it must always be kept in mind that the water used in making the concrete, or which

may come in contact with it, may prove to be a very influential factor for harmful results.

Then, if one is given a concrete failure to diagnose one must look for a reason under cement, aggregate, workmanship and water. As stated, the easiest explanation is to look for some fault in the cement, but if, as has been suggested, the cement had passed the usual tests, then other reasons must be found. To find them requires that the investigator shall first know conditions and causes of failures, but these will not be further discussed except to point out the way to the reason; then the investigator must attack the problem with an open mind—that is, he must not jump at a conclusion and expect to be able to work out an explanation around that conclusion. Then it is even more essential that the investigator shall have had an opportunity to learn of results of tests, or to make tests that will enable him to judge the probable actions from the characteristics of the aggregates. Also, a very careful examination of the concrete in place is generally an absolute necessity.

To attempt to outline a plan for procedure in this examination would be folly, since each individual case has conditions that are decidedly its own, and the law of probabilities makes possible many combinations of causes which can only be worked out as one would solve any involved research problem. Clues must be sought, and a sharp eye, a quick ear, and a questioning tongue must be alert to grasp a clue and pursue it to a definite ending. If any one should be skeptical of the efficiency of this method, it is possible that a few illustrative cases, selected from a large number of diagnoses, may convince that one that the method has proven and can prove successful, far more than is generally expected.

ILLUSTRATIVE CASES OF FAILURE.

Failure due to Water.—A highway was being built and the plans called for several new concrete culverts. All but one of these culverts "set up," or hardened, nicely. This one did not, and yet the same cement, sand and stone had been used as in all the other culverts. A reason for the failure of the one culvert was desired.

It was found that the brook which flowed through this

particular culvert passed in its course the plant of a company engaged in the manufacture of medicine from herbs. The refuse from the plant so loaded the water with organic matter that it prevented a proper hardening of the concrete. No one connected with the construction of the culvert knew that water so contaminated would have that effect, and the complaint came in that the cement was not acting properly. The brook was temporarily diverted, other water was used and the concrete acted normally. A condition had existed there which would not have existed in city water, but which is found frequently in the country, especially in wooded sections. To detect the cause one had to know that certain elements, such as tannic acid, alkali salts, etc., foreign to most waters, do affect concrete.

Failure due to Sand.—For an illustration of the effect of a poor quality of sand, consider the following case:

A cry of alarm came in from an engineer that the concrete in an important bridge abutment had been in place for over two weeks, and that in attempting to remove the forms it was found that the concrete was still so soft that it could be cut out with a knife. A brand of cement new to the work had been used, and the blame was of course placed on it. An examination showed that a footing for the abutment had been made of the brand of cement first used on the work and that, although ten days older, the concrete could easily be cut with a knife. The evidence eliminated the cement.

Examination of the sand showed it to be a well-graded, sharp sand, but a clue was furnished by some yellow-coated grains. The investigator had already had considerable experience with similar sand grains. A trip to the sand bank showed just what he expected to find. The bank had not been stripped of a top layer of yellow-coated sand, which gives a sand with which it is mixed a tendency to very materially delay the hardening of the concrete in which it is used. In time, usually several months, the concrete will harden and there is ultimately no harmful effect apparent. Lack of knowledge of this peculiar quality had caused an investigator of another piece of work to tear it out and rebuild.

Failure due to Stone.—A very unusual condition existed in another case, but it emphasizes strongly the need of following

clues. A concrete wall was apparently disintegrating due, as the engineers believed, to free lime in the cement. In various places on the face of this wall there appeared what can best be described as "blisters." By prying off these blisters there were produced small cones about 6 in. in diameter and 3 in. in height and in the apex of each could be seen a small, yellowish-white spot about the size of a small marble. The trouble was clearly not a case of free lime in the cement. One unusually large blister enabled the author to dig out from the apex a soft stone about $1\frac{1}{2}$ in. in size. This stone did indicate the presence of free lime, and after a few weeks on the author's desk slaked into a powder. Examination of the stone composing the coarse aggregate soon brought out the following:

The crushed stone all came from the same quarry, but some came by a steam railroad and some by an electric railway. No concrete made by the former showed blisters. Concrete made from stone delivered by trolley did, but why? It seems that in the course of transportation by the trolley route, the stone was conveyed in waste dump-cars, across the grounds of the company owning the quarry and conducting a plant in which limestone is an essential raw material. These dump-cars had not been cleaned carefully, and to the good stone were added some small quantities of stone that had been through a chemical process and were on the verge of disintegration. In the concrete their expansive force blistered the face. By forbidding further deliveries by trolley the trouble was stopped.

These may be considered extraordinary cases, but it is the out-of-the-ordinary that makes trouble; if they were not the extraordinary they would probably have been guarded against. They are, at least, typical of points for which one must look if one would explain failures in concrete.

Failure due to Workmanship.—Failures due to poor workmanship are seen so often that instead of citing particular cases it will be sufficient to briefly note some causes or results.

Failures from faulty design are shown in the mode of failure. The lack of proper proportioning may be clearly seen in a fractured surface; the grading of the aggregates is also similarly noted; and poor mixing and improperly placed concrete readily show themselves. All these are evident to an eye

trained to know good or bad concrete. The failure to take care of laitance is made apparent by the seams that are bound to result from such a failure.

A concrete may be dense but not sound and hard, and "sounding" with a hammer will show up this characteristic. Too wet a mixture with fine sand or silt, or a crusher dust used as fine aggregate, may be a cause. It may be sound and hard and yet may be poor for certain uses because it is too porous. By scratching the face of unbroken concrete with a dull instrument, one may sometimes judge of proportions used and, in its early stage, can also judge somewhat as to the rate of hardening. A pocket glass in the field and a microscope in the laboratory help materially in determining the density, and in approximating the ratio of cement and fine aggregate to the coarse aggregates. The naked eye is all that is needed to observe concrete spoiled by sweepings of sawdust, shavings or blocks, or by waste carelessly dropped into a form. Lack of protection to fresh concrete from the sun or unusual heat may be noted by a "dried-out" and rapidly dusting surface, and from frost by a flaked and scaly surface.

INFLUENCE OF EXTERNAL FORCES.

Where concrete has apparently been good for a considerable period and has then begun to disintegrate, the reason for the failure must be sought in the character of the disintegration, whether it may be due to changes in the elements making up the concrete or to some external elements or forces that have entered into it. If due to an internal influence alone the fact will be noticed by the granular breaking up of the concrete. If due to an external force, such forces as sea water, alkali salts or electrolysis will be under suspicion as being responsible if the concrete has been under the influence of any of them. For many years all the failures of concrete were considered as being due to the formation of certain chemical relations; but as it is commonly acknowledged at present that an impermeable concrete will stand in sea water as well as elsewhere, although subjected frequently to more severe actions than other concrete, its failures are due frequently to the same causes as those of other concrete and therefore the same examinations should give

the same results. Similarly, since electrolysis is considered as a cause for the breaking down of concrete, the liability of concrete being affected by its influence will depend upon whether the concrete comes under the influence of an electric current. If it does, then its influence needs to be examined. If it does not, then the examination should also follow the usual method.

CONCLUSIONS.

The value of laboratory tests and analyses should not be overlooked. While perhaps not giving as definite information as a field inspection, they should be used to the fullest extent to help establish the strength or weakness of a reasonable theory for the cause of failure. Where time will permit, laboratory tests of concrete made of the aggregates under as nearly as possible the same conditions, will give results that should aid in determining the fault in the original. Test specimens so made and treated should give practically the same results, and when compared with test specimens made under ideal conditions or with standard materials, should give the investigator the reasons for the failure.

All means possible should be used by an investigator in making his examination, and his conclusions must be drawn only after he has considered the failure from every probable cause with reason and fairness. The object of the examination is either to place responsibility or to guard against future failures, and right conclusions are the only ones that should be drawn if justice to either objective is to be given.

DISCUSSION.

MR. H. S. MATTIMORE.—It must be admitted that cement Mr. Mattimore. has been a sadly abused product. The average engineer not skilled in testing has and is still likely to first look for cement as the cause of failure in concrete. But if the following striking illustration is used as an example, it can be seen that there must be a large proportion of cement of a more or less questionable quality being put on the market.

The New York State Highway Commission recently installed a cement testing laboratory. The use of cement on the highways of this state has increased to such a quantity that it is quite an important market for the producers. For the first three months of this year, from January to April, there were 96,000 bbl. of cement tested. Of this amount 51,800 bbl., or approximately 54 per cent, were rejected. During the three months from April to June, 342,000 bbl. were tested and of this amount 18,300 bbl., or about 5 per cent, were rejected. The one bright outlook in this example is the phenomenal improvement in product in such a short duration of time.

The tests during both periods were performed by the same operators, all of whom have had extensive practical experience in cement testing. The methods and tests were strictly according to the specifications of the Society. Thus, with the cement meeting standard requirements, cement might be said to be almost eliminated as a factor in concrete failure, but when such is not the case it is a doubtful factor, almost impossible to detect.

Numerous examples of well-graded sharp sand with yellow coating on the grains, detectable with a high-power hand-glass or microscope, have come under the observation of the writer. This material so far as known at present is confined to certain limited areas of the state of New York. Although, as Mr. Greenman states, the concrete built with this sand as an aggregate may harden in time, the resulting concrete is not of a high

Mr. Mattimore. grade and therefore such sand is rejected for use on the New York state highways.

One point not mentioned under the heading of failures due to coarse aggregate, is want of bond between both stone and gravel and the finer aggregate; this is caused in stone by a slight film of dust surrounding the stone, or in the softer grades of limestone by a dust coating collecting during the crushing operations. In gravels from many sections of New York, there is a slight loam coating which prevents proper bond. This often occurs in gravels where the amount of free loam is not large enough to be objectionable, and is readily detected by the ease with which the coarse aggregate is freed from the finer matrix.

Mr. Lesley.

MR. R. W. LESLEY.—Mr. Mattimore has brought out a very interesting point, and I think one rather familiar to most of us. Mr. Mattimore's laboratories were started and at the end of three months' operation 50 per cent of the cements was condemned. The laboratory was in existence six months, and in the last three months about 5 per cent was condemned. It is a well-known fact that in the last four or five years no cement works have been built, and that most of the present works date back 20 or 30 years, and that therefore any striking defect in cement could not occur. Would it be possible that this peculiarity in testing was due to what we all, as cement manufacturers and cement experts, happen to know, that a new laboratory has its failings as well as the cement it tests?

Mr. Chatburn.

MR. G. R. CHATBURN.—I was just wondering, when the gentleman was speaking about 50 per cent of the cement being rejected, upon what ground it was rejected. On three different occasions, at least, I have been called in consultation after cement had been rejected for unsoundness, and have found the cement in each case to be perfectly sound within the limits of the ordinary tests for soundness. In every instance some three or four weeks had elapsed after the test upon which it was rejected before I was called in. We all know that new cement is likely to be unsound; great demand or insufficient storage capacity often induces factories to ship cement almost immediately after grinding. I do not believe any cement should be rejected upon a first steaming or boiling test unless it is absolutely certain that the cement has had time for proper aging. I wonder if the

cements rejected (50 per cent is a large amount to reject) might not have been rejected for unsoundness as determined by the boiling or steaming test, the cement not having been sufficiently cured. Mr. Chatburn.

Speaking of aggregates, I might mention a case that came up a short time ago. The floor of a cellar which had been constructed of cinder concrete proved to be unsatisfactory. I tried the same kind of cinders in some sidewalk experiments; sections 4 ft. square, having various aggregates, proportions and thicknesses, were constructed. The blocks made of cinder concrete were entirely disintegrated in two years' time. Why cinder concrete which seems to be all right for dry floors should be bad in a cellar and in a sidewalk was the question we set ourselves for solution. Believing this to be largely a chemical question the matter was placed in charge of Mr. George Borrowman. The results of his investigation were published in the *Journal of Industrial and Engineering Chemistry*, June, 1912. The analysis of the cinders, which were from bituminous coal, was as follows:

SiO ₂	36.87	per cent
Total Fe as Fe ₂ O ₃	21.16	"
Al ₂ O ₃	17.40	"
CaO.....	10.96	"
MgO.....	0.83	"
S.....	0.60	"
SO ₃	5.82	"
Undetermined.....	5.37	"

A number of blocks of concrete were made up of cinders in varying proportions and their behavior observed. At the end of a few weeks these showed signs of disintegration. The breaks and cracks seemed to originate at points where oxidized cinders existed. Mr. Borrowman states that it is "probable that oxidation of iron and sulfur produced internal stress and consequent cracking aided by the general weakness produced by the diffusion of soluble sulfates. This theory was strengthened by finding that the efflorescence found on the surface of the disintegrating concrete was ferrous sulfate." Cinders from the same stock were exposed to air and moisture until free from soluble sulfate when "under parallel conditions no disturbance took place."

Mr. Chatburn. He then concludes "that cinders with much sulfide and sulfate sulfur are likely to give unsatisfactory results, especially if there is much coke or porous material present; also that such material may be improved if allowed to weather with occasional washing until the ferrous iron and sulfur have been oxidized and leached out."

Mr. Johnson. MR. N. C. JOHNSON.—I should like to show on the screen some photomicrographs of cement mortars and concretes which I chance to have with me. These photographs might partially answer the question as to the possibility of determining the amount of cement that has been used in a concrete, and might also serve to give a new and better idea as to the interrelation of sand and cement in the matrix, in which the stone of concrete is embedded. The stone itself can not be shown, as even a small portion of the surface of a single piece of stone would more than fill the entire field of the microscope, this field being about $\frac{1}{16}$ in. in diameter, measured at the surface of the specimen under examination.

This photograph (Fig. 1) shows the appearance of a 1 : 3 mortar of Portland cement and standard Ottawa sand. The round character of the grains is very noticeable, while between the sand grains are the particles of unhydrated or partially hydrated cement. Many of these particles that appear single are, in reality, a central particle wrapped, surrounded and probably waterproofed by smaller particles, so that hydration is not progressive, as is usually taken for granted.

The next photograph (Fig. 2) shows a similar mortar made with standard sand, 28 days old at the time it was photographed. A sharp grain is to be seen in the center of the field. Black spots in various places indicate holes, or voids.

The next photograph (Fig. 3) shows the matrix of a sample of 1 : 2 : 4 concrete, five years old. The same characteristics are evident in this photograph as in those of the standard sand mortars.

The appearance of neat cement under the microscope is indicated in the last photograph (Fig. 4). The percentage of unhydrated particles is astonishing at first sight, and gives one a new understanding of the wastefulness of our ordinary mixing of this excellent engineering material.

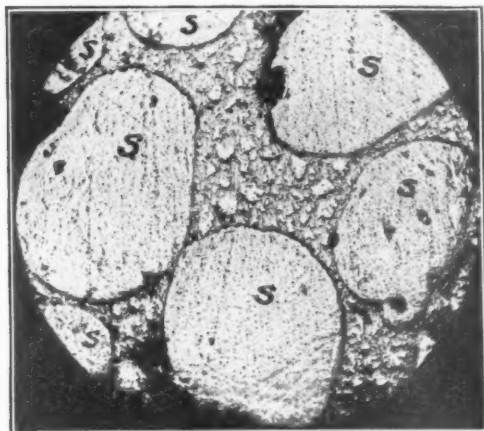


FIG. 1.—1 : 3 Mortar with Standard Ottawa Sand.
Magnification, $\times 40$.

Sand grains indicated by "S." Particles showing
between sand grains are those of unhydrated
Portland cement.

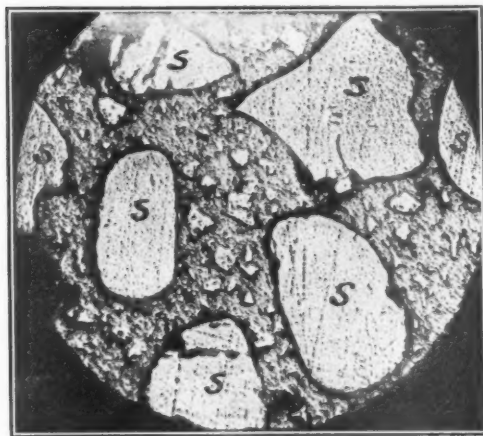


FIG. 2.—1 : 3 Mortar with Standard Ottawa Sand.
Magnification, $\times 40$.

Sand grains indicated by "S." Particles showing
between sand grains are those of unhydrated
Portland cement.

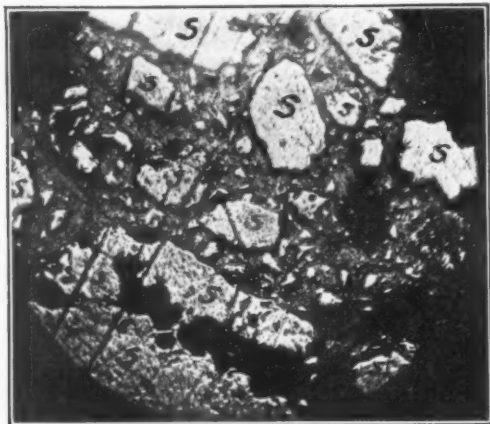


FIG. 3.—1 : 2 : 4 Concrete, Five years old.

Magnification, $\times 40$.

Sand grains are indicated by "S." Particles between sand grains are those of unhydrated Portland cement. Irregular dark spots are minute voids. This is a photograph of the mortar from between the stones (gravel) of the concrete.

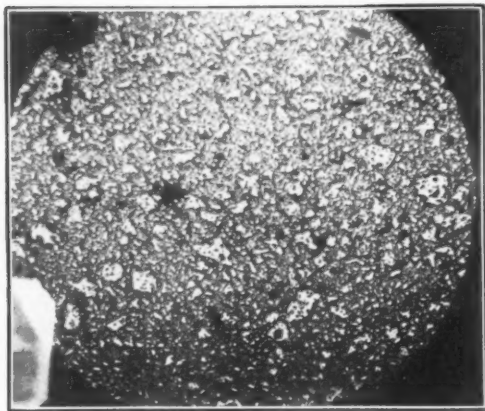


FIG. 4.—Neat Portland-Cement Mortar, 28 days old.

Magnification, $\times 40$.

All particles in this photograph are cement particles, either wholly or partially unhydrated.

Each cement grain and each sand grain in these photographs has its own shadow. On the screen, the sand grains are as large as glacial boulders, and the cement grains, as large as cobblestones, so this shadow is perhaps not surprising, but when we consider their actual size it opens new conceptions to us as to the interaction and interrelation of the materials that enter into concrete. Of course these specimens are carefully ground and polished, so that this relief will show, but this preparation serves only to bring out the true values of the material, and not to obscure them or to alter their nature. Further, while in the photographs the particles of cement appear white, they are, in reality, black, or green, or a greenish brown, this color reversal being due to the use of reflected light in photographing. Also, these specimens are not thin sections but are polished on one surface only after the manner of steel examinations in metallographic work. Mr. Johnson.

As the sand grains are chemically inert, unless fine enough and of proper composition to act as puzzoline material, the determination of the percentage of unhydrated cement is easily made, by using a rectangular coordinate screen, either with the photographing or with the visual microscope. The visual microscope is perhaps the more accurate of the two, as it reveals color values, and makes evident slight differences that the reflecting microscope fails to show. By examining several portions of a specimen, and by taking specimens from several portions of a structure, as accurate a determination of the value of a concrete can be made as is possible in examining steels by similar methods. I have personally been studying concrete and cement in this way for over two years; and I am endeavoring to make such classification of my results as to reduce this examination to a science. In the hands of a skilful operator, it should yield excellent results.

MR. A. M. JOHNSON.—In case the concrete is friable could you procure such a surface? Mr. A. M. Johnson

MR. JOHNSON.—I believe I could. I have examined a considerable number of chalky concretes, some of which were extremely bad, and have obtained very good results. The preparing of the specimen, however, is a matter of delicacy and skill, for although the sand grains and untouched cement particles remain hard, the chalky matrix and partially hydrated cement particles cut down so fast that a smooth surface is very difficult Mr. Johnson.

Mr. Johnson. to obtain. As a microscope focus is reckoned in ten-thousandths of an inch, and as a smooth surface is a prerequisite to polishing, it can be surmised that the process is neither easy nor simple, but it can be used by a skilled operator.

Mr. Wig. MR. R. J. WIG.—I think the objection to both these methods is the impracticability of getting a sample of the concrete which is truly representative of the mass. We find that unless very large samples are taken, and with considerable judgment, the results are not dependable.

In so far as the actual amount of cement in the sample is concerned, that can be determined readily by two methods. If the concrete contains limestone aggregate, the cement content may be approximately obtained by determining the soluble silica, unless the sand used contains considerable soluble silica, which is seldom the case. Most sands contain a negligible quantity of soluble silica, and where limestone is not used as an aggregate the results obtained by the soluble-silica method can be checked by determining the content of lime. If the surface of the concrete has been exposed to the weather, or to percolating waters, some of the lime may have leached out, and the results obtained will be too low. Interior concrete, however, would not be subject to such changes. The cement content is calculated by assuming 20 or 21 per cent of soluble silica and 63 per cent of lime in the original cement, or if the brand used is known, a more exact assumption can be made. The value of any results, however, is dependent upon the sampling and quartering down of the sample to laboratory size, so that it truly represents the concrete in the structure. Having determined the quantity of soluble silica, the amount of cement in the concrete can be determined by calculations, as previously stated.

Mr. Humphrey. MR. RICHARD L. HUMPHREY.—My experience in the use of acid in determining the proportions of a defective concrete, leads me to the belief that, while it is a very crude method at best, if the proportions of the concrete depart considerably from the requirements of the specification—for example, where a 1 : 1 : 8 mixture was used instead of a 1 : 2 : 4 mixture—and provided the aggregate is not soluble, a reasonably close determination of the actual proportions may be obtained. It is a very difficult problem to take concrete not uniformly hardened

and break it up into its component parts, and say, with any degree of certainty, how much is cement and how much is fine and coarse aggregate. The problem becomes even more difficult when the aggregates are soluble in the acid. But when intelligently done, it is possible to determine the approximate proportions of the concrete. Mr. Humphrey.

MR. MATTIMORE.—In reply to Mr. Chatburn, I would say that our soundness tests for tests are in most cases made up two weeks after the cement is sampled. This is especially true where a failure on the first test requires a check. Mr. Mattimore.

In rejections, where large quantities of cement were in question, check tests have been made in the presence of representatives of the company producing the cement, and some times the tests have been made by the representatives themselves. In all cases the rejection has held.

MR. R. S. GREENMAN (*Author's closure, by letter*).—Mr. Mattimore's discussion well illustrates the problems a new laboratory has to meet. That fact that, during the second three months of its work, the percentage of rejections was only five, would surely indicate that the testing had reached a rational basis, for no one acquainted with the cement industry can believe that 54 per cent of the product would fail to reach standard requirements when properly tested. The yellow-coated sand to which Mr. Mattimore refers is evidently not the same sand I have had experience with, for the concrete to which I referred is as fine a piece of concrete as one would wish to see. Mr. Greenman.

I believe the method of determining the proportions of materials in concrete by means of photomicrographs as proposed by Mr. Johnson gives much promise of being a great aid in the examination of concrete, but, as Mr. Wig has suggested, there will always be the difficulty of selecting a small sample that is a truly representative sample of the concrete. Nevertheless, by use of several carefully selected samples an average result might be secured. It is to be hoped that some method more certain than the so-called "acid method" can be developed.

STRENGTH OF LIME MORTAR.¹

BY W. E. EMLEY AND S. E. YOUNG.

SUMMARY.

In this paper is described an attempt to measure the compressive, tensile, shearing, and transverse strengths of lime mortar, and to ascertain how these values are affected by the numerous variables met with in practice, such as: chemical composition of the lime; kind and amount of sand; size and shape of test specimen; conditions under which the test specimen is stored; the effect of added materials, etc.

The results indicate that the strength developed by a specimen of lime mortar is affected by too many variables to be capable of accurate measurement in the laboratory, and that even if this were possible, there is no assurance that the results obtained would be comparable with those which might be expected in practice.

¹ By permission of the Director of the National Bureau of Standards.

STRENGTH OF LIME MORTAR.

BY W. E. EMLEY AND S. E. YOUNG.

Until very recently, the compressive strength of lime mortar was considered of so little importance as to be hardly worthy of investigation. Since a cubic foot of masonry weighs about 150 lb., and the pressure exerted by a wall due to its own weight is only about 1 lb. per sq. in. for every foot of height, almost any kind of mortar can withstand this compression, and lime mortar, by centuries of satisfactory use, established its reputation so well that it was never questioned. Recently, however, lime has been forced to compete with Portland cement, a material which is known to be much stronger than lime, but which, in some markets at least, costs more when laid in the wall. Whether the increased strength of a Portland-cement mortar would be worth the increased cost became a debatable question.

The tendency towards increasing the height of buildings has also brought the compressive strength of lime mortar into question, especially if the building is to be used as a store room or shop, in which the walls must carry considerably more than their own weight.

The practice of breaking the joints when laying brick, taken in connection with wind pressure, settling of foundations, and similar causes, may set up transverse, shearing, or even tensile stresses in the mortar.

During the past five years a large number of miscellaneous tests have been made, with a view to ascertaining: (1) the strength a lime mortar may be expected to develop in practice; (2) the best means for measuring this strength; (3) how much one lime may be expected to vary from another; and (4) the cause of this variation.

The hardening of lime mortar is due to the action of the carbon dioxide of the air; that is, it begins on the surface and works towards the interior. Therefore, the rate of set of the mortar, and consequently its strength at any age, will depend upon the size and shape of the specimen, and the condition

of the atmosphere in which it is stored. The skill and experience of the person who makes the specimen is of the greatest importance in any work of this kind, and it will be shown later that this is especially true when dealing with lime. For these reasons it is necessary to note all of the details of manipulation, and the results cannot be expected to agree very closely. Any law deduced from them must be considered general, and cannot be applied with very great precision.

COMPRESSIVE AND TENSILE STRENGTHS OF MORTARS MADE FROM COMMERCIAL LIMES.

In order to get an idea of the values of the quantities with which we have to deal, a number of samples of commercial limes and hydrates were tested. These samples were collected by the writer at the plants,¹ and shipped to the laboratory in air-tight containers. Ten 2-in. cubes and 5 briquettes were made from each sample in the following manner:² Two and one-half pounds of lime were put in a galvanized iron box, and enough water was added to slake to a rich putty. After action had ceased the putty was covered with $7\frac{1}{2}$ lb. of Merrimac River sand. The box was sealed air-tight and permitted to stand 24 hours. The sand was then mixed into the putty by hand, and the mortar molded. The specimens were left in the molds two days, during the first of which they were covered with damp cloths. They were then removed from the molds, stored in a damp closet for 1 month, and in a warm, dry room for 5 months, when they were broken.

The results obtained are given in Table I. These results show that the compressive strength of 1 : 3 mortar made from high-calcium lime is about 100 lb. per sq. in.; from a dolomitic lime, over 300 lb. per sq. in. In short, lime made from dolomite is about three times as strong as lime made from limestone. Of course, other factors must be considered: for instance, that the high-calcium limes did not have enough sand; that the consistency was judged by eye, and probably varied considerably.

¹ For descriptions of these plants, see Emley, "The Manufacture of Lime," *Technologic Paper No. 16*, Bureau of Standards.

² Emley, "Methods of Testing Lime," *Transactions, Natl. Lime Mfrs. Assoc.*, Vol. 9 (1911).

But it will be shown later that this ratio is generally true even when these factors have been equalized as far as possible.

Let us now consider what effect the variation of these factors will have upon the strength of the mortar. This can best be done by assuming an arbitrary value for each of the factors to be investigated, and then vary them one at a time. Any change of strength will then be dependent upon the particular factor varied.

TABLE I.—COMPRESSIVE AND TENSILE STRENGTHS OF MORTARS MADE FROM COMMERCIAL LIMES AND THREE PARTS RIVER SAND, SIX MONTHS OLD.

Quicklime.			Hydrated Lime.		
CaO, per cent.	Compressive Strength, lb. per sq. in. (Ave. of 10.)	Tensile Strength, lb. per sq. in. (Ave. of 5.)	Ca(OH) ₂ , per cent.	Compressive Strength, lb. per sq. in. (Ave. of 10.)	Tensile Strength, lb. per sq. in. (Ave. of 5.)
98.41	72	28	92.98	61	21
98.13	90	43	91.77	80	31
98.13	48	28	88.78	153	46
97.78	104	30	88.42	128	48
97.65	98	40	87.78	95	19
96.22	118	29	82.46	188	47
95.81	156	36	74.38	130	28
95.38	73	21	61.44	148	22
95.01	129	33	57.27	222	29
94.64	156	45	54.05	165	37
94.09	85	25	52.19	259	67
93.84	147	63			
92.56	154	31			
92.45	156	36			
92.23	92	36			
89.29	144	40			
86.23	126	40			
84.51	119	43			
84.41	113	..			
60.87	186	44			
60.40	280	63			
58.10	313	50			
56.95	391	84			
56.93	319	41			
56.33	315	59			
55.98	363	93			
55.06	303	71			

Two kinds of lime were prepared by burning samples of limestone and dolomite respectively. These limes were mixed with different proportions of water, and the consistencies of the resultant pastes measured by means of the plunger viscosimeter.¹ It was found that the first lime required 184 per cent of water to make a paste of standard consistency, while

¹ Emley, "Methods of Testing Lime," *Transactions, Natl. Lime Mfrs. Assoc.*, Vol. 9 (1911).

the second took 180 per cent. To these pastes were then added varying proportions of sand, and it was found that each lime required $3\frac{1}{2}$ parts of sand to give mortars of standard consistency.¹

EFFECT OF AGE ON COMPRESSIVE STRENGTH.

To determine the effect of age on the compressive strength 2-in. cubes were made of these mortars, and stored in air in the laboratory.

The results are shown in Table II. Each of these figures

TABLE II.—VARIATION OF COMPRESSIVE STRENGTH WITH AGE.

(AVERAGE OF 3 TESTS.)

Age, days.	Compressive Strength, lb. per sq. in.			Age, days.	Compressive Strength, lb. per sq. in.		
	High- Calcium.	Dolomitic.	Check Test (Dolomitic).		High- Calcium.	Dolomitic.	Check Test (Dolomitic).
5	34	30	79	123	152
6	..	38	...	60	70	128	...
10	35	90	63	109	307
11	42	120	76	134	...
12	49	36	...	150	...	168	...
13	..	43	...	180	108	217	308
14	..	63	...	210	112	233	...
15	60	270	311
20	..	77	...	360	278

is the average of three 2-in. cubes. It was expected that the mortars would show minimum strengths between 10 and 15 days, because it was found by previous experiments that at this age the cubes had lost most of their free water, and had taken up very little carbon dioxide. Such did not prove to be the case, but both limes showed a decrease of strength at about 90 days. In order to check this, another lot of cubes was made of the dolomitic mortar, but these did not show any such decrease. It is presumed, therefore, that the loss of strength originally noted is probably due to the atmospheric conditions at the time the cubes were tested.

¹ Emley and Young, "Crushing Strength of Lime Mortar," *Transactions, Natl. Lime Mfrs. Assoc.*, Vol. 11, p. 254 (1913).

EFFECT OF SIZE AND SHAPE OF SPECIMEN ON COMPRESSIVE STRENGTH.

The size and shape of a specimen of lime mortar are important factors influencing its strength, because the setting of the mortar depends upon the action of the carbon dioxide of the air. The size and shape are measures of the distance which this gas will have to penetrate in order to attack the interior of the mass.

The rate at which carbon dioxide penetrates can be shown visibly if the mortar is made with a 1-per-cent aqueous solution of wool blue R (an organic dye made by the Berlin Aniline Works). This dye colors the calcium carbonate blue, but leaves the hydrate colorless. If a specimen is made of this mortar, and is broken at any age, that part of it which has set will be colored blue, and can be readily seen.¹

It is well known that if we have two columns of the same cross-sectional area and shape, but one longer than the other, the shorter one will be able to support the greater load, which, of course, implies a greater load per unit of area. This relation has been expressed by the statement that the compressive strength varies as the ratio between the square root of the area and the height.² Evidently, if the specimens are cubes, this ratio will be unity, independent of the size of the cube. A number of cubes of different sizes were made of the two mortars mentioned above, and tested when 90 days old.

The results are shown in Table III (a). It will be noted that the compressive strengths of the different cubes are not the same, and it must, therefore, be concluded that lime mortar does not follow the rule. There are two obvious reasons why this is the case: (1) The larger cube presents less area to the action of the carbon dioxide, in proportion to its volume; (2) the carbon dioxide must penetrate a greater distance in order to attack the lime in the center of the larger cube. It follows, therefore, that the larger cube should be the weaker at any given age, which was found to be the case. This would probably not hold if the specimens were old enough to be fully

¹ Emley, "Method of Measuring Rate of Set of Lime Mortar," *Transactions, Am. Ceramic Soc.*, Vol. 16 (1914).

² Martens, "Handbuch der Materialenkunde für den Maschinenbau."

carbonated. Under such circumstances, the cubes would be homogeneous, and should follow the law; that is, their compressive strengths per unit of area should be independent of their sizes.

One of the causes for the variation from the rule can be eliminated if the specimens are so designed that the exposed area (total exterior area minus area of base) is maintained constant, while the ratio between the square root of the area and the height is varied. Such specimens were made from each

TABLE III.—VARIATION OF COMPRESSIVE STRENGTH WITH (a) SIZE OF CUBES, (b) RATIO OF $\sqrt{\text{AREA}}$ TO HEIGHT (EXPOSED AREA CONSTANT), AND (c) EXPOSED AREA (CROSS-SECTIONAL AREA AND HEIGHT CONSTANT).

(AVERAGE OF 3 TESTS.)

(a) SIZE OF CUBES.			(b) RATIO OF $\sqrt{\text{AREA}}$ TO HEIGHT.			(c) EXPOSED AREA.			
Size of Cube, in.	Compressive Strength, lb. per sq. in.		Ratio.	Compressive Strength, lb. per sq. in.		Shape of Cross-Section.	Exposed Area, sq. in.	Compressive Strength, lb. per sq. in.	
	High-Cal-cium.	Dolo-mitic.		High-Cal-cium.	Dolo-mitic.			High-Cal-cium.	Dolo-mitic.
0.5	106	1068	1 : 5	79	172	Triangle...	22.24	101	174
1	153	222	1 : 2.5	86	120	Square...	20.00	106	207
1.5	132	225	1 : 1	103	192	Pentagon...	19.27	98	201
2	119	211	2.5 : 1	313	457	Hexagon...	18.88	104	182
6	54	58	5 : 1	Circle....	18.17	95	168

of the mortars. They were right prisms, having square cross-sections and the following dimensions:

RATIO.	SIDE OF SQUARE, IN.	HEIGHT, IN.
1 : 5.....	0.975	4.875
1 : 2.5.....	1.35	3.37
1 : 1.....	2.00	2.00
2.5 : 1.....	2.77	1.11
5 : 1.....	3.33	0.667

Total exposed area (not including bottom), 20 sq. in.

These specimens were broken when 90 days old, with the results shown in Table III (b). Those specimens which had a ratio of 1 : 5 bent slightly while they were being stored, so that they failed by flexure, instead of by compression. Those having a ratio of 5 : 1 were subjected to the full capacity of

the testing machine (about 900 lb. per sq. in.) without any noticeable drop of the beam. But when the load was taken off, the specimens were found to be completely disintegrated. It will be noted that this shape of specimen corresponds more closely to the shape of the mortar in a brick wall. It therefore seems reasonable to suspect that the strength actually developed by a mortar will be much greater than that indicated by a 2-in. cube of the same material.

Further information in regard to the effect of shape on the compressive strength can be obtained if the specimens are so designed that the area in compression and the height are both maintained constant, while the area exposed to the action of air is varied. The specimens were made in the form of right prisms, having a height of 2 in., and a cross-sectional area of 4 sq. in. The exposed area was varied by changing the shape of the cross-section, as follows:

SHAPE OF SECTION.	LENGTH OF SIDE, IN.	EXPOSED AREA, SQ. IN.
Triangular.....	3.04	22.24
Square.....	2.00	20.00
Pentagonal.....	1.527	19.27
Hexagonal.....	1.24	18.88
Circular.....	2.26 (diameter)	18.17

These specimens were made of each of the two mortars noted above, and were broken when 90 days old, with the results shown in Table III (c). These results are of no value, because the variation of exposed area was not sufficiently great. Any effect it may have had was masked by the errors of the experiment.

EFFECT OF SAND ON COMPRESSIVE STRENGTH.

Another factor which influences the strength of lime mortar is the sand which it contains—both the amount of sand, and the size of its grains. When a neat lime paste is permitted to set, it shrinks so much as to cause pronounced cracking, and generally disintegration. The addition of a certain proportion of sand will overcome this tendency to crack, and produce a mortar of measurable strength. Any further addition of sand will act merely as a diluent of the binding material and hence weaken the mortar.

Two-inch cubes were made of the lime pastes mentioned above, to which had been added different proportions of standard Ottawa sand. These cubes were crushed when 90 days old, with the results shown in Table IV (a). The influence of the amount of sand is found to fulfil expectations.

It is a commonly accepted statement that if a number of mortars are made from the same lime and the same proportion of sand, that mortar will be the strongest whose sand grains are of such sizes as to give the greatest density. This rule may not apply to lime mortar, because great density implies less porosity, and hence greater resistance to penetration of carbon dioxide. The added strength due to greater

TABLE IV.—VARIATION OF COMPRESSIVE STRENGTH WITH (a) AMOUNT OF SAND, AND (b) SIZE OF SAND GRAINS.

(AVERAGE OF 3 TESTS.)

(a) AMOUNT OF SAND.			(b) SIZE OF GRAINS.		
Sand, parts.	Compressive Strength, lb. per sq. in.		Size of Sand Grains, between Meshes Nos.	Compressive Strength, lb. per sq. in.	
	High- Calcium.	Dolomitic.		High- Calcium.	Dolomitic.
$\frac{1}{4}$	273	372	10—20	98	166
1	151	267	20—30	118	214
2	116	217	30—40	138	312
3	112	202	40—60	186	335
4	116	203	60—80	260	444

density may be counterbalanced by the loss of strength due to incomplete carbonation.

Two-inch cubes were made of the lime pastes mentioned above, to which had been added $3\frac{1}{2}$ parts of Ottawa sand screened to the sizes indicated in Table IV (b). Evidently the strength does increase with the density, in spite of the tendency towards incomplete carbonation.

EFFECT OF CONSISTENCY ON COMPRESSIVE STRENGTH.

Another important factor which must be considered is the consistency of the mortar. If the mortar is too dry, it may be difficult to obtain a bond between the particles, and hence the specimen may be weak. If too wet, it will be diffi-

cult to make the specimen homogeneous, and when the water evaporates the resultant high porosity may be expected to cause low strength. The amount of sand, as well as the amount of water, affects the consistency of the mortar. Hence the three component series, lime-sand-water, was investigated.

Two-inch cubes were made by varying proportions of a dolomitic hydrate, run-of-mine Ottawa sand, and water. The

TABLE V.—VARIATION OF COMPRESSIVE STRENGTH WITH CONSISTENCY.
(AVERAGE OF 3 TESTS.)

Composition.			Consistency, seconds.	Compressive Strength, lb. per sq. in.	Composition.			Consistency, seconds.	Compressive Strength, lb. per sq. in.
Lime, per cent.	Water, per cent.	Sand, per cent.			Lime, per cent.	Water, per cent.	Sand, per cent.		
65	35	0	tk	310	40	20	40	tk	18
65	30	5	tk	78	35	45	20	0.39	80
60	40	0	tk	283	35	40	25	0.37	78
60	35	5	tk	308	35	35	30	0.56	150
60	20	20	tk	20	35	30	35	1.30	203
55	45	0	0.78	148	35	25	40	tk	345
55	40	5	tk	205	30	35	35	0.40	123
55	35	10	tk	235	30	30	40	0.56	115
55	30	15	tk	65	30	25	45	1.31	180
50	50	0	0.46	98	30	20	50	tk	205
50	45	5	0.78	148	25	30	45	0.37	145
50	40	10	1.52	203	25	25	50	0.43	158
50	35	15	tk	255	25	20	55	0.74	213
50	30	20	tk	48	25	15	60	tk	40
50	20	30	tk	320	20	25	55	0.36	103
45	50	5	0.41	93	20	20	60	0.57	210
45	45	10	0.42	100	20	15	65	tk	255
45	40	15	0.63	145	15	25	60	0.34	65
45	35	20	tk	233	15	20	65	0.39	75
45	30	25	tk	283	15	15	70	0.62	175
45	25	30	tk	500	15	10	75	tk	38
40	50	10	0.33	68	15	5	80	tk	20
40	45	15	0.40	80	10	20	70	0.35	103
40	40	20	0.81	135	10	15	75	tk	120
40	35	25	1.70	140	10	10	80	tk	78
40	30	30	tk	233	5	5	90	0.35	10
40	25	35	tk	295					

tk = too thick to measure.

cubes were stored in air in the room for 60 days and broken, with the results shown in Table V.

The consistencies of the mortars were measured by the plunger viscosimeter, and are stated in the table as the time required to pull the plunger out of the paste—the thicker the paste, the longer the time.¹ As was expected, the thicker the consistency of the mortar, the greater its strength, up to the

¹Emley, "Effect of Consistency and Amount of Sand on the Properties of Lime Mortars," *Transactions, Am. Ceramic Soc.*, Vol. 16 (1914).

point where the mixture is no longer a mortar, but becomes a damp powder, when the strength falls off abruptly.

One of the peculiarities of lime is brought out by these results. At about the consistency ordinarily used, any slight change in the composition of a mortar causes a large change in its consistency. This property makes it difficult to prepare mortars of standard consistency; but on the other hand, it enables the operator to judge by handling how closely a mortar approaches the standard, with a fair degree of accuracy.

EFFECT OF CONDITION OF ATMOSPHERE ON COMPRESSIVE STRENGTH.

Another important factor which must be considered is the condition of the atmosphere in which the specimens of lime mortar are stored. The setting of the mortar depends upon the chemical reaction between calcium hydroxide and carbon dioxide. This reaction will probably follow the ordinary laws; that is, it will be influenced by the temperature, and by the partial pressure of the carbon dioxide.

Moreover, it is known that a certain amount of water is necessary to give the reaction measurable velocity, although too much water has the same effect as not enough water.

We may expect, therefore, that lime will set more rapidly in warm weather than in cold. The partial pressure of carbon dioxide may be of importance, especially if a large amount of mortar is stored in a closed room. The rate of reaction is very slow at best, so that it may be possible to keep up the supply of this reagent by diffusion; but it is reasonable to suppose that the carbon dioxide can be supplied more quickly by convection—as when the specimens are exposed to wind.

The water content is a very important factor. It is well known that a lime mortar will not set so rapidly if the air is either too wet or too dry; the best results being obtained when the mortar is alternately soaked and permitted to dry out. It is a matter of general experience that if a lime mortar is tested for compressive strength, the results will be low if the specimen is wet. This factor is so important that it may overshadow the others and really determine the strength of the mortar.

Moreover, if the mortar is wet too soon after it is made, it may be weakened by the handling or abrasion.

A mortar was made of a dolomitic hydrate with 3 parts of run-of-mine Ottawa sand and 97.2 per cent of water (based on the lime). Two-inch cubes were made up, half of them being stored in air in the room, the other half on the roof of the building. When 60 days old, the cubes were tested for compressive strength. A new batch of the same composition was made up once a week for 17 weeks. The weather conditions, such as

TABLE VI.—VARIATION OF COMPRESSIVE STRENGTH WITH CONDITION OF ATMOSPHERE.

(AVERAGE OF 3 TESTS.)

Date		Average Temperature, deg. Fahr.		Number of Days		Times Frozen.	Times Rained Upon.	Average Humidity, per cent.	Compressive Strength, lb. per sq. in.	
Made.	Tested.	Out-side.	In-side.	after storing on roof to first rain.	after last rain to time of testing.				Out-side.	In-side.
June 11	Aug. 10	82	77	2	2	0	19	67	227	135
19	18	81	77	2	1	0	20	67	310	127
26	25	80	76	1	3	0	19	66	189	102
July 3	Sept. 1	78	75	2	3	0	16	66	249	167
10	8	79	77	1	0	0	16	65	83	122
17	15	78	76	2	3	0	15	64	355	150
24	22	75	76	0	1	0	18	64	199	143
31	29	73	75	4	7	0	17	64	258	175
Aug. 7	Oct. 6	70	74	0	4	0	17	66	263	198
14	13	68	73	0	2	0	15	66	259	174
21	20	66	72	4	1	0	17	65	179	132
28	27	64	71	4	2	1	20	65	193	152
Sept. 4	Nov. 3	59	70	0	3	5	20	66	261	192
11	10	57	68	1	0	7	22	64	133	233
18	17	54	67	0	1	11	22	64	164	227
25	24	54	68	0	1	11	23	63	292	261
Oct. 2	Dec. 1	52	67	5	0	12	26	62	...	163

temperature, humidity, etc., were recorded every day during the test.

The results are shown in Table VI. The variation in compressive strength shown by the specimens stored indoors is entirely too large to be accounted for by the slight changes of humidity and temperature to which these specimens were subjected. A certain amount of variation in the quality of the lime was unavoidable. This, together with changes of the moisture-content of the sand and the amount of dissolved substances in the water, caused noticeable variations of consistency even

though the mortars were all of apparently the same composition. It is believed that these variations in consistency are chiefly responsible for the variations of strength. The atmospheric conditions outdoors changed through much wider limits, and their effects are noticeable in most cases. Thus the figures show that a high temperature is generally accompanied by high strength, which agrees with the reasoning given above. The number of times the specimens were rained on appears to have little influence, although this number varied only from 15 to 20, which change may have been too small to make itself felt. Repeated freezing and thawing is likewise without noticeable effect. The time at which the first and last rains occur are very important factors. Specimens which were rained on when less than 5 days old show materially lower strengths, as do also specimens which were tested while still wet. A combination of these circumstances accounts for the remarkably low strength of the specimens made up on July 10. On the other hand, if a specimen is exposed to hot, dry weather for several days just after it is made up, it is apt to dry out rapidly and unevenly, so that it may disintegrate. This occurred with the specimens made on October 2.

COMPRESSIVE AND TENSILE STRENGTHS OF VARIOUS COMMERCIAL LIMES.

All the results indicate that if a comparison between the compressive strengths of different limes is desired, it is absolutely essential that all of the conditions of the test be identical. It is generally impossible to use the same amount of sand for every lime and get mortars of uniform consistency. But the figures of Table IV (*a*) indicate that the amount of sand is of little importance, provided that it is more than twice the amount of lime. Hence, it will be safer to vary the amount of sand, and maintain the same consistency throughout.

A number of samples of commercial limes were mixed with enough water to give pastes whose consistency figure was 0.6 second when measured in the plunger viscosimeter. To these was added enough standard Ottawa sand to raise the consistency figure of the resultant mortars to 1 second. Briquettes

TABLE VII.—COMPRESSIVE AND TENSILE STRENGTHS OF COMMERCIAL LIMES.

(AVERAGE OF 5 TESTS.)

HIGH-CALCIUM QUICKLIMES.

Im- purities.	Analysis of Lime as Received, Percentage of						Per- centage of Water used in Mortar, based on Lime.	Parts of Sand used in Mortar, based on Lime	Com- pressive Strength, lb. per sq. in.	Tensile Strength, lb. per sq. in.
	CaO.	Ca(OH) ₂ .	CaCO ₃ .	MgO.	Mg(OH) ₂ .	H ₂ O (free).				
1.20	84.35	12.00	1.55	1.22	262	4.00	79	20
11.30	70.40	15.38	0.75	1.92	170	3.25	98	22
0.82	96.65	1.56	0.45	0.90	300	3.00	60	23
0.90	90.03	6.08	1.59	1.75	456	5.00	80	31
0.97	86.51	7.93	4.05	0.35	245	4.25	72	29
0.93	96.02	1.48	0.93	0.72	426	3.50	95	30
2.27	91.51	4.40	1.73	0.98	208	4.25	73	21
1.27	93.18	3.00	2.41	0.17	506	5.00	41	23
1.03	44.73	51.06	2.57	1.03	160	4.00	106	35
2.30	87.59	5.67	4.18	0.71	339	5.00	78	27
0.76	82.02	11.80	0.75	4.55	213	2.50	99	37
1.16	80.79	14.18	1.23	2.50	178	3.25	108	26

HIGH-CALCIUM HYDRATES.

0.99	4.38	81.61	12.45	0.42	106	3.50	99	33
3.65	0.92	86.83	7.66	1.28	96	3.00	63	22
1.96	91.67	4.09	0.70	1.17	96	2.25	198	56
6.34	84.72	4.64	3.03	0.93	76	1.75	185	41
1.76	90.58	3.11	2.73	0.88	103	3.50	90	24
3.00	91.07	4.45	1.41	2.98	82	1.25	177	34
1.32	91.21	5.23	0.41	1.64	96	2.75	150	45
1.68	95.90	1.57	0.84	0.38	76	1.50	172	47
1.59	88.85	4.27	2.00	3.06	84	3.00	191	43
0.96	94.51	1.30	2.13	1.33	96	2.50	141	45
3.96	0.76	77.86	14.20	3.42	92	2.50	208	50

DOLOMITIC QUICKLIMES.

2.47	59.15	6.41	0.80	31.61	181	2.75	169	40
0.47	50.26	2.26	6.84	40.62	170	2.75	192	42
2.81	69.00	15.01	4.41	8.97	122	2.50	123	37
1.67	74.21	7.98	1.50	14.75	248	4.25	87	34
6.52	69.92	11.06	0.54	12.07	91	1.00	145	43
3.41	76.04	2.59	1.32	16.83	113	2.00	88	34

DOLOMITIC HYDRATES.

4.59	65.12	7.00	22.34	1.10	96	3.00	111	24
1.10	57.76	5.34	26.27	7.15	96	2.75	498	113
0.90	61.13	3.02	30.91	4.51	104	3.00	360	101
3.32	61.73	1.80	32.27	1.19	87	2.00	341	62
7.52	2.63	58.54	1.52	29.80	92	2.75	191	47
1.29	62.52	2.23	26.05	9.34	96	2.25	745	147
3.55	62.89	2.09	27.77	3.29	87	3.00	182	37
5.17	36.43	22.30	33.92	6.82	72	1.50	77	35

TABLE VIII.—VARIATION OF COMPRESSIVE AND TENSILE STRENGTHS OF LIMES WITH IMPURITIES ADDED.

(AVERAGE OF 3 TESTS.)

Impurities Added to Stone, kind and per cent.	Percentage of Water, based on Lime.	Parts of Sand, based on Lime.	Age, days.	Com- pressive Strength, lb. persq. in.	Tensile Strength, lb. persq. in.
None.....	239	2.75	60	..	28
SiO ₂ —1.0.....	223	3.90	60	77	28
SiO ₂ —5.0.....	178	4.00	60	67	33
SiO ₂ —10.0.....	170	3.20	60	78	29
Fe ₂ O ₃ —1.0.....	257	4.25	60	78	31
Fe ₂ O ₃ —5.0.....	223	6.60	60	95	..
Fe ₂ O ₃ —10.0.....	186	4.05	60	87	..
Fe ₂ O ₃ —25.0.....	144	4.75	60	131	..
Al ₂ O ₃ —1.0.....	300	3.05	60	42	22
Al ₂ O ₃ —10.0.....	355	6.50	60	104	46
Al ₂ O ₃ —25.0.....	186	4.50	60	362	101
MgO—1.0.....	271	4.00	60	50	31
MgO—10.0.....	203	4.50	60	73	..
Kaolin—5.0.....	207	5.00	60	58	..
Kaolin—10.0.....	194	4.00	60	70	..

Impurities Added Stone, Lime or Hydrate, kind and per cent.	Parts of Sand, based on Lime.	Age, days.	Tensile Strength, lb. per sq. in.		
			Stone.	Lime.	Hydrate.
Al ₂ (SO ₄) ₃ —0.1.....	3	7	12	20	19
Al ₂ (SO ₄) ₃ —1.0.....	3	7	10	28	21
Al ₂ (SO ₄) ₃ —10.0.....	3	7	14	45	17
AlPO ₄ —0.1.....	3	7	6	15	17
AlPO ₄ —1.0.....	3	7	13	12	21
AlPO ₄ —10.0.....	3	7	8	6	15
Calcium Aluminate—0.1.....	3	7	13	15	19
Calcium Aluminate—1.0.....	3	7	10	17	19
Calcium Aluminate—10.0.....	3	7	16	14	30
Bauxite—0.1.....	3	7	11	16	15
Bauxite—1.0.....	3	7	12	16	16
Bauxite—10.0.....	3	7	13	14	15
None.....	3	7	10	10	14

Impurities Added to Hydrate, kind and per cent.	Parts of Sand, based on Lime.	Age, days.	Com- pressive Strength, lb. persq. in.	Age, days.	Tensile Strength, lb. persq. in.
Agar-agar—0.05.....	3	7	58	1	16
Agar-agar—0.10.....	3	7	41	1	17
Agar-agar—1.00.....	3	7	69	1	17
Al ₂ (SO ₄) ₃ —1.00.....	3	7	185	1	19
Al ₂ (SO ₄) ₃ —3.00.....	3	7	157	1	18
Al ₂ (SO ₄) ₃ —5.00.....	3	7	164	1	19

	Age, days.	Com- pressive Strength, lb. persq. in.		Com- pressive Strength, lb. persq. in.	Tensile Strength, lb. persq. in.
Dolomitic Hydrate, all Mg as Mg(OH) ₂ ; 74 per cent water, 2½ parts sand.	6 10 13 14 16 20 31 60	41 57 80 93 108 134 196 221	Dolomitic Lime con- taining Sr instead of Mg; 4 parts sand, 90 days old.	210	53

and 2-in. cubes were made of these mortars, and tested after aging for 90 days in the laboratory.

The results are shown in Table VII.¹ From the method observed in making the specimens, these results can be considered as fairly comparable, and it will be noted that the dolomitic limes and hydrates are stronger than the high-calcium compounds.

To conclude this part of the subject, a number of miscellaneous results are given in Table VIII. These tests were designed primarily to show the influence of certain impurities on the strength.

A study of all the results given indicates (1) that the compressive strength of a lime mortar is of value for comparison only when all the conditions under which it was made and stored are known; (2) that it is useless to specify any figure for compressive strength unless the method of measuring it is also given in detail, as well as the consistency and the condition of atmosphere in which the specimens are stored; (3) that the real strength of the mortar as used is probably much greater than the measured strength, so that this latter value can be only comparative at best, and cannot be used in any engineering calculations (see Table III (b)).

STRENGTH OF LIME-CEMENT MORTARS.

In order to improve the working qualities of Portland-cement mortars, it has become common practice to add small proportions of hydrated lime. This practice has brought about a demand for more information, such as: "How much lime can be used with safety?"; "Is it safe to use lime in foundation work, or under water?"; "Does the magnesia in a dolomitic hydrate act similarly to that contained in the cement itself?". In order to answer these questions, a number of specimens of mortar were made up containing different proportions of either high-calcium or dolomitic hydrate, and Portland cement. The consistency used was the standard adopted for lime—0.6 second for neat pastes and 1 second for mortars, measured by the

¹ Emley, "Results of Testing Lime," *Transactions, Natl. Lime Mfrs. Assoc.*, Vol. 11, p. 77 (1913).

plunger viscosimeter. This consistency is much thinner than that ordinarily used for cement, which probably accounts for the low strengths shown by the cement specimens.

The results, in each case an average of three specimens, are shown in Table IX (Plate II). Consideration of these figures leads to the following conclusions: (1) The addition of small amounts of hydrated lime does produce a weaker mortar, but this decrease of strength is hardly worth noticing if the volume of the lime is one-third or less than that of the cement; (2) this statement holds true whether the mortar is stored in air or under water; (3) the results obtained with dolomitic hydrate were at least equally as satisfactory as those high with calcium hydrate.

It must be emphasized that these results were obtained with mortars whose consistency was much thinner than that ordinarily used for Portland cement. However, other investigators, using different consistencies, seem to have arrived at about the same conclusions.¹

SHEARING STRENGTH OF LIME MORTARS.

The compressive, tensile and transverse strengths of lime mortar offer no great difficulties to their measurement by the ordinary means. The shearing strength, however, has not been sufficiently investigated to make certain just what influence various factors may have upon it. Some preliminary work was necessary, therefore, to determine the best method of measuring the shearing strength. An instrument was built to grip the two ends of a bar of lime mortar and hold them rigidly, while the part of the bar between the supports is sheared out by a load equally distributed over its entire area. It will be noted that the area in shear is twice the cross-sectional area of the bar.

A number of specimens were made up of a high-calcium hydrate with 3 parts run-of-mine Ottawa sand. These were stored in the room for 30 days, and their shearing strengths

¹ Lazell, "Tests of Cement-Lime Mortars," *Transactions, Natl. Lime Mfrs. Assoc.*, Vol. 9 (1911); Spackman, "Effect of Hydrated Lime on Change in Volume and Strength of Mortars and Concretes," *Transactions, Natl. Lime Mfrs. Assoc.*, Vol. 12 (1914).

TABLE IX.—COMPRESSIVE AND TENSILE STRENGTHS OF HYDRATED-LIME AND PORTLAND-CEMENT MORTARS.

(AVERAGE OF 3 TESTS.)

ALL STRENGTHS IN POUNDS PER SQUARE INCH.

Composition, per cent.	Age.	Stored in Air on Roof.						Stored Under Water.						Percentage of Water Used, based on	
		High-Calcium.			Dolomitic.			High-Calcium.			Dolomitic.				
		1 : 3 Mortar.		Neat.	1 : 3 Mortar.		Neat.	1 : 3 Mortar.		Neat.	1 : 3 Mortar.		Neat.		
		Compressive Strength.	Tensile Strength.	Tensile Strength.	Compressive Strength.	Tensile Strength.	Tensile Strength.	Compressive Strength.	Tensile Strength.	Tensile Strength.	Compressive Strength.	Tensile Strength.	Tensile Strength.	Neat Paste.	Mortar.
Cement, 100 . . .	7 da.	1047	161	284	1047	161	284	703	125	436	703	125	436	27.6	69.5
	28 da.	1712	228	331	1712	226	331	1358	202	507	1358	202	507		
	3 mo.	1656	257	753	1656	257	753	2363	276	577	2363	276	577		
	6 mo.	2316	356	483	2316	356	483	2654	343	522	2654	314	522		
	1 yr.	2941	479	495	2941	479	495	2716	228	495	2716	228	495		
Cement, 95 Lime, 5	7 da.	892	126	266	512	82	219	783	116	302	668	97	296	32.6	73.6
	28 da.	1508	213	578	1172	190	374	1640	173	417	1404	174	358		
	3 mo.	1426	220	418	1065	269	379	2530	263	457	2500	257	512		
	6 mo.	2353	482	143	2000	286	420	2594	289	523	2545	212	504		
	1 yr.	2229	405	149	2134	235	79	2753	230	529	2386	189	491		
Cement, 90 Lime, 10	7 da.	653	110	271	703	95	256	642	105	270	656	110	319	34.5	69.0
	28 da.	1116	156	235	990	148	302	1589	181	390	1405	163	345		
	3 mo.	1316	301	275	1896	276	190	2020	286	420	2348	217	447		
	6 mo.	1524	276	269	1415	272	423	2556	231	429	2440	227	475		
	1 yr.	2504	431	146	1758	154	119	2659	208	505	2478	199	472		
Cement, 85 Lime, 15	7 da.	703	104	209	580	89	201	637	102	259	674	78	186	39.0	60.2
	28 da.	958	159	238	1149	164	413	1507	167	367	1010	121	326		
	3 mo.	1286	248	301	1249	178	152	2238	235	449	1741	182	368		
	6 mo.	1475	288	211	1755	267	229	2307	266	429	2124	184	392		
	1 yr.	2401	377	183	2126	205	83	2567	274	386	1888	185	417		
Cement, 75 Lime, 25	7 da.	624	72	137	622	92	233	651	83	183	564	62	172	42.5	60.0
	28 da.	737	132	159	1031	154	371	1345	126	249	1034	109	249		
	3 mo.	1190	254	238	1128	162	175	2030	199	410	1634	154	319		
	6 mo.	1346	257	219	1282	234	489	2018	208	260	1894	148	352		
	1 yr.	1805	384	169	1862	122	71	2185	193	422	2022	154	475		
Cement, 50 Lime, 50	7 da.	356	52	54	263	64	79	311	40	62	259	22	51	62.0	74.7
	28 da.	533	71	58	351	65	61	500	77	104	421	60	106		
	3 mo.	817	195	255	735	141	87	748	121	196	738	93	133		
	6 mo.	953	204	69	...	53	...	846	130	204	866	102	186		
	1 yr.	1215	297	1072	130	208	1181	104	217		
Lime, 100	7 da.	44	20	20	86	35	15	93.3	98.7
	28 da.	...	46	22	145	96	56		
	3 mo.	248	75	66		
	6 mo.	121		
	1 yr.	251	110	78		

measured. The factors varied were the width and depth of the bar, and the length of the span (the distance between the supports).

The results are given in Table X, and are, in each case, the average of three specimens. Unfortunately, these figures do not bring out the desired information, which was obtained rather by observation while the specimens were being tested. It was observed that if the area in compression is less than the area in shear, the bar will fail by compression—the center part will be crushed and will display the usual pyramidal form. The area in compression can be increased by making the span longer, and this of course will not increase the area in shear. It was found,

TABLE X.—VARIATION OF SHEARING STRENGTH WITH SPAN, DEPTH AND WIDTH OF SPECIMEN.

(AVERAGE OF 3 TESTS.)

Depth = 1 in. Width = 1 in.		Width = 1 in. Span = 1 in.		Span = 1 in. Depth = 1 in.	
Span, in.	Shearing Strength, lb. per sq. in.	Depth, in.	Shearing Strength, lb. per sq. in.	Width, in.	Shearing Strength, lb. per sq. in.
0.5	76	0.5	148	0.5	106
1	65	1	61	1	70
2	69	1.5	49	1.5	70
4	59	2	47	2	75
6	64	2.5	30	2.5	76
		3	22	3	88

however, that when the span was more than 4 in. (width, 1 in., depth, 1 in.) the bar tended to shear at one end first. This is probably due to the fact that it is almost impossible to apply the load exactly parallel to the planes of shear, and the effect of any eccentric loading is exaggerated by the long span. In order to measure the true shearing strength, therefore, it is necessary to use such a span that the area in compression will be not less than one nor more than two times the area in shear. When so measured, the shearing strength per unit of area was found to be independent of the depth or width of the bar, except in so far as these dimensions affect the rate of carbonation of the mortar.

TABLE XI.—VARIATION OF THE STRENGTHS OF LIME MORTAR.
(AVERAGE OF 3 TESTS.)

ALL STRENGTHS IN POUNDS PER SQUARE INCH.

TRANSVERSE STRENGTH COMPUTED FROM $R = \frac{3 W l}{2 b d^2}$.

Variables.	Compressive Strength.	Tensile Strength.	Shearing Strength.	Transverse Strength.	
KIND OF LIME.					
Normal Consistency; 3 parts Sand; Run-of-mine Ottawa Sand; Stored in Room; 90 days old.	High-Calcium..... Dolomitic.....	403 78	69 21	82 58	146 42
CONSISTENCY.					
Dolomitic Hydrate; 3 parts Sand; Run-of-mine Ottawa Sand; Stored in Room; 90 days old.	Normal minus 5 per cent water..... Normal..... Normal plus 5 per cent water.....	349 403 224	75 69 66	112 82 94	137 146 115
AMOUNT OF SAND.					
Dolomitic Hydrate; Normal Consistency; Run-of-mine Ottawa Sand; Stored in Room; 90 days old.	1 part..... 3 parts..... 5 parts..... 10 parts..... 20 parts.....	724 403 158 110 22	76 69 19 too weak too weak	114 82 39 37 too weak	203 146 64 57 24
KIND OF SAND.					
Dolomitic Hydrate; Normal Consistency; 3 parts Sand; Stored in Room; 90 days old.	Run-of-mine Ottawa..... German Standard..... Standard Ottawa..... 200-mesh Flint.....	403 262 246 122	69 59 48 32	82 77 89	146 120 115 65
STORAGE.					
Dolomitic Hydrate; Normal Consistency; 3 parts Sand; Run-of-mine Ottawa Sand; 90 days old.	Stored in Room..... Stored on Roof..... Stored in Damp Closet....	403 556 340	69 120 81	82 235 108	146 448 126
AGE.					
Dolomitic Hydrate; Normal Consistency; 3 parts Sand; Run-of-mine Ottawa Sand; Stored in Room.	15 days..... 30 days..... 45 days..... 60 days..... 75 days..... 90 days..... 120 days.....	130 139 242 191 297 403 325	21 26 31 63 76 69 69	67 59 78 98 100 82 67	66 70 96 104 130 146 129
COMPOSITION.					
Dolomitic Hydrate; Normal Consistency; 3 parts Sand; Run-of-mine Ottawa Sand; Stored in Room; 90 days old.	100 per cent Lime..... 75 per cent Lime, 25 per cent Plaster..... 100 per cent Plaster..... 75 per cent Lime, 25 per cent Cement..... 100 per cent Cement..... 97.5 per cent Lime, 2.5 per cent Hair.....	403 379 1460 847 743 311	69 61 116 160 157 36	82 107 175 51 368 71	146 179 444 216 198 99

COMPARISON OF THE DIFFERENT STRENGTHS OF LIME MORTAR.

It was noted above that the size and shape of the specimen have a marked influence on its strength, because of their effect upon the rate of carbonation. It is necessary, therefore, that all specimens used for measuring compressive, tensile, shearing, or transverse strength, be of the same size and shape. This was accomplished by making all the specimens in the form of bars 1 by 1 by 6 in. By the use of wool blue R, it was found that the composition of such a bar is practically uniform up to about $\frac{1}{2}$ in. from either end.

After the bars were stored for the required length of time, the compressive strength was measured on a 1-in. cube sawed out of the center of the bar. For tensile strength, a piece about 3 in. long, cut from the center of the bar, was placed in a briquette mold, and the necessary lugs were built on with plaster of Paris. The shearing strength was measured with a span of 3 in., so that the area in compression was $1\frac{1}{2}$ times the area in shear. A span of 4 in. was used in measuring the transverse strength, and the modulus of rupture was calculated by the customary formula.

The influence of different factors upon these strengths are shown by the results given in Table XI. A general review of these figures shows that the average ratios of compressive strength to tensile strength, to shearing strength, and to transverse strength, are 1 : 0.17, 1 : 0.26, and 1 : 0.39, respectively.

CONCLUSIONS.

In conclusion, we may state that in order to use the strength of lime mortar as a basis of comparison, we must specify every minute detail of manipulation used in measuring the strength. The extreme difficulty of maintaining uniform atmospheric conditions over long periods of time (which is absolutely essential if comparative results are to be obtained) practically precludes the specification of any definite figure for the desirable strength of a lime. It might be possible to eliminate the effect of the atmospheric conditions, if we could select some lime as a standard. Parallel series of specimens, one made of standard lime and the other of the lime to be tested,

both subjected to the same conditions throughout, would give a means of comparing the unknown lime with the standard. Unfortunately, all commercial limes vary more or less in their properties, owing to the variations in the stone itself, and in the temperature at which it is burned.

The strength of a lime mortar depends mainly on the atmospheric conditions, and on the size and shape of the specimens. It is impossible to duplicate these practical conditions in the laboratory, and our present knowledge of the subject does not warrant the establishment of any definite relation between the strength as measured in the laboratory and that which may be expected in practice.

REPORT ON A PERMEABILITY TEST FOR PAINTS AND VARNISHES.

BY A. M. MUCKENFUSS.

SUMMARY

Object.—The object of the paper is to ascertain the accuracy with which the permeability of paints and varnishes to moisture could be measured, and the extent to which such measurements at different periods were an index to durability.

Methods.—The permeability was obtained in a constant-temperature room by placing the paint, spread on hardened paper, wire cloth, or cement, over a pan of water and under a weighed dish of calcium chloride for 24 hours, all parts being sealed with mercury, so that the dish could only gain in weight ("actual permeability") by water-vapor passing up through the coating. The coated panel could be easily removed, exposed, and returned for successive measurements. The exposure could be ordinary, but was generally artificial. For this purpose, the sun's light and heat were imitated with electric lights, and by similar methods, rain and cold were reproduced, as well as regular weather changes, in a specially constructed tank.

Principal Results.—Implement paints showed sharp contrasts in 3 days of artificial exposure; outside whites, in 81 days; drying oils, in 107 days; metallic coverings, in 114 days; exterior enamels, in 13 days; cement coatings, in 2 days. Equally sharp contrasts were obtained among exterior and interior varnishes. The effect of small amounts of rosin was early manifest in the permeability changes. Permeability was proved to be a property sensitive to all conditions. The perviousness of a coating was shown to be much greater outwards than inwards. Each weathering force had its own

peculiar effect. Several oils were indicated to have definite permeabilities.

Conclusions.—The various characteristics of a coating are continually changing during exposure. Such changes are dependent on one another. A measurement of one indicates the alteration of the other characteristics, or the extent to which the coating is aging.

REPORT ON A PERMEABILITY TEST FOR PAINTS AND VARNISHES.

By A. M. MUCKENFUSS.

A partial report on this method of testing has been made.¹ In this, the apparatus and process were described briefly. It is proposed now to give a full account of the mode of procedure and of such results as were not reported or not completely reported at that time.

(A) THE PERMEABILITY APPARATUS.

The Nature of the Test.—The process consists in determining the permeability of the fresh paint or varnish coating or after drying for a definite number of days under uniform conditions. The panel is then exposed on the roof or given artificial exposure, after which its permeability or porosity is again measured. This routine of exposure followed by measurement is repeated as often as desired, the measurements always being made in a room maintained at a standard temperature. Such a room will be described later.

The Testing Device.—Figs. 1, 2 and 3 (Plate III), give a part sectional plan, vertical sectional view of the complete testing apparatus, and a horizontal section of the base alone, respectively. Fig. 4 is a photograph of the various parts and Fig. 5 gives a view of five sets of the apparatus in the constant-temperature room. In these figures, like parts are designated by the same reference numeral, except the numerals on Fig. 5. The base (1), $10\frac{13}{16}$ in. in diameter, is made of cast iron, and contains an inner and an outer groove (13), with an outer rim (2), an inner rim (3) and a central rim (4). In these grooves, mercury rests to the depth of about $\frac{3}{8}$ in., the base being level. In the center of the base is a pan (5), in which water to the depth roughly of $\frac{1}{2}$ in. is placed. A concave ring (6) is made

¹ Muckenfuss, "Preliminary Report upon a Practical Accelerated Test for Paints and Varnishes," *Journal of Industrial and Engineering Chemistry*, Vol. 5, p. 535 (1913). Copies of this report in pamphlet form may be obtained gratis from the Patton Paint Co., Milwaukee, Wis.

of tin and the two parts soldered together. The lower part (12) of the ring is beveled back and the whole given two coats of red lead and oil before using. The outer diameter of these rings is $9\frac{3}{16}$ in. and their height 1 in. A clamping ring (8) fits exactly into the upper part of the concave ring (6). Cloth, paper, wire cloth, or wood veneer is placed between the two rings and the two are clamped together by means of short pieces of wire solder (11) or iron wire, through holes shown in Fig. 1, the whole making a panel (23). The dovetailing of the rings into each other stretches the above porous surfaces tightly. The diameter of the surface is $7\frac{3}{4}$ in. and its area nearly 47 sq. in. A plain ring (9) corresponds to the lower part of the concave ring (6) and may be soldered in like manner to the clamping ring (8), making a convex ring, and when clamped to the concave ring, making a double panel. With this arrangement, as shown in Fig. 2, the porous surface (7) upon which the paint, oil, or varnish is spread, may be raised, inverted, and set on the base with the painted side in the reverse position.

The ring (14) is made of galvanized iron wire, $\frac{1}{4}$ in. in diameter. This rests upon the painted panel and upon it rests the disk (18), made of galvanized-iron wire cloth, 4 mesh and 0.063 in. wire. Upon the disk lies the weighed dish (19), vertical and horizontal views of which are shown in Fig. 6. The dish is made of glass and is 6 in. in diameter and 1 in. high. Its cover is like that of an ordinary desiccator and is ground to fit in the same way. There are two styles for the dish, as shown in Fig. 6. The lower of the two was the one used in this research. Fused calcium chloride, in lumps about $\frac{1}{4}$ in. in diameter, is placed in the dish to about half its depth and the ground joints are coated lightly with a vaseline-rubber-paraffin composition to render them air-tight when covered. Finally, there rests in the outer groove a glass cover (10), which is $9\frac{3}{4}$ in. in diameter and $3\frac{1}{8}$ in. in height. A hole (20) is bored in the center of this cover and by means of two rubber washers and two nuts, the end of a $\frac{1}{4}$ -in. brass tube (16), 6 in. long, is clamped air-tightly to the cover. The tube, which serves as a handle, is bent around as shown in Figs. 2, 4, and 5, joining at its other end to a calcium-chloride tube (21), 6-in. size, filled half with fused calcium-chloride, the half next to the outer end being plugged with cotton. Just as the tube (16) main-

PLATE III.
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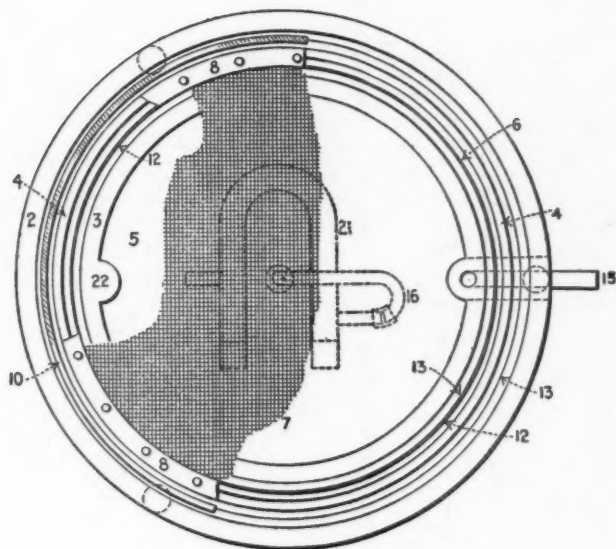


FIG. 1.—Part Sectional Plan.

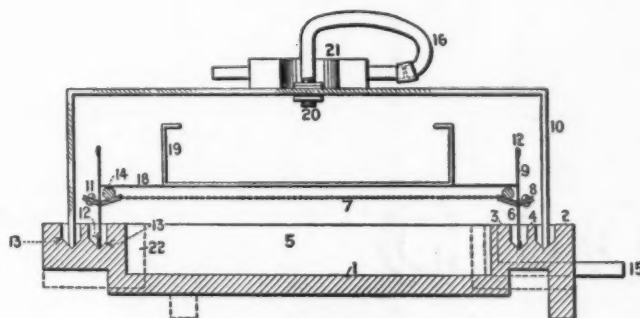


FIG. 2.—Vertical Section of Complete Apparatus.

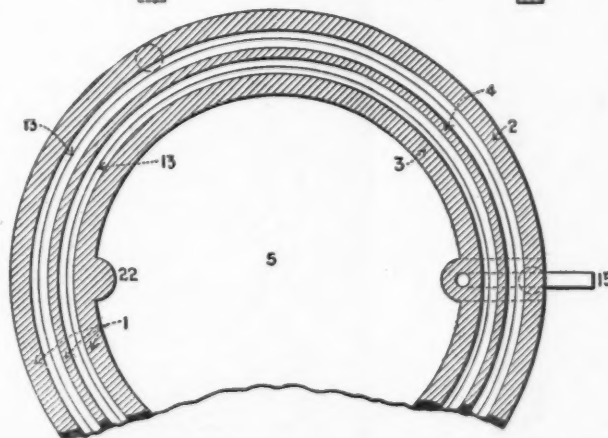


FIG. 3.—Horizontal Section of Base.

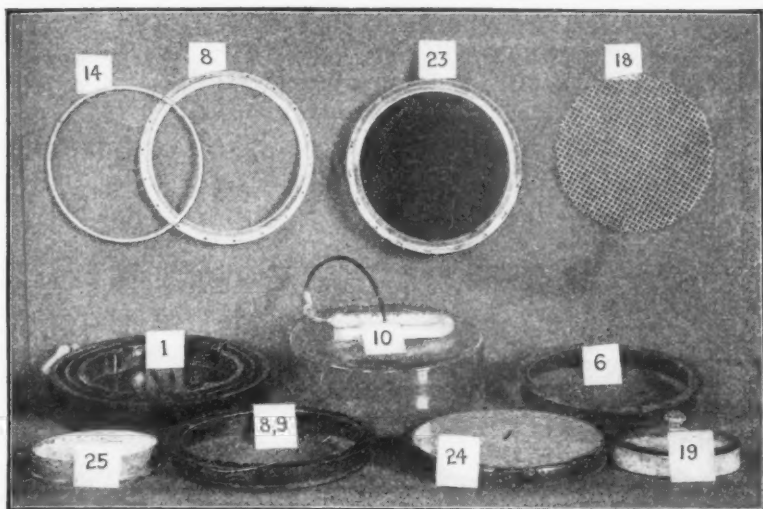


FIG. 4.—Various Parts of Testing Device.

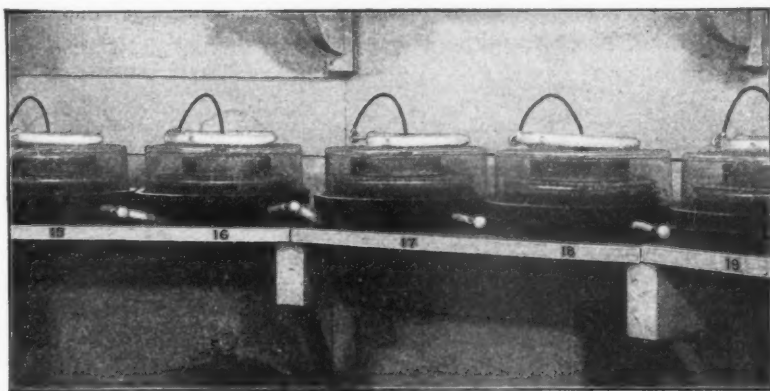


FIG. 5.—Five Sets of Testing Device in the Dry Constant-Temperature Room of the Patton Paint Co.

tains atmospheric pressure in the sealed space above the porous surface (7), so also the tube (15) maintains the same pressure in the sealed space under the surface. The latter tube is attached to a drying tube, plugged with cotton, as shown in Figs. 4 and 5. Opposite the tube (15) is a space (22) in the casting for a tube similar to the tube (15), if it is desired to pass any gas under the porous surface (7). It should be further

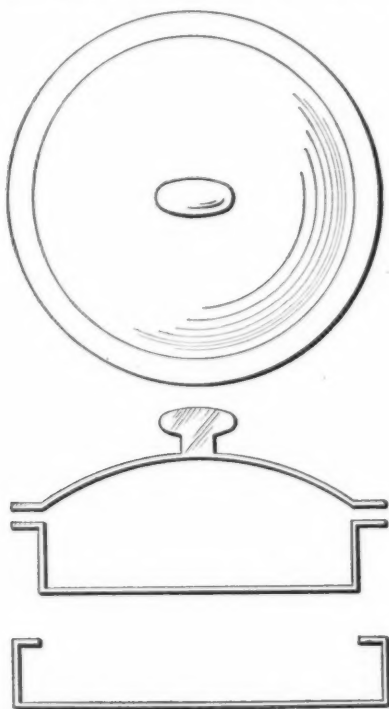


FIG. 6.—Weighed Dish.

explained that an enameled iron pan (24) lies in the inner groove in place of the panel (23), when the apparatus is not in use, and that an unweighed dish (25) contains the regular absorbent, to be used in place of the weighed dish (19), when the panel is in place but no measurement is being made. So much for the construction of the form of apparatus at present in use.

Making a Measurement.—To make a measurement of permeability in the same manner as for the results exhibited

in this paper, spread the paint or varnish thinly and evenly on the weighed panel (23) with a flat badger-hair brush, $1\frac{1}{2}$ in. wide, and wipe off all the liquid not in contact with the porous surface. After drying sufficiently in the constant-temperature room, apply another coat, and after this has dried, weigh the panel. The increase, expressed in grams, represents the weight of the particular coating. The panel is now ready for the initial measurement of its permeability. Place it upon the base (1) and over it set the wire ring (14). Upon this place the disk (18) and on it set the unweighed dish (25). After placing the cover (10) in position, let stand from 12 to 24 hours, as shown in Fig. 5. Then note the hour and minute and remove cover (10) and the unweighed dish (25). As quickly as possible place the weighed dish (19) on disk (18), remove its top, and replace cover (10). The next day at the same hour and minute remove cover (10), replace top to weighed dish (19), and weigh the same, expressing the increase in milligrams. If the calcium chloride gets wet during a measurement, remove the cover (10) before the expiration of 24 hours, or when the wetting becomes noticeable, and calculate the result for 24 hours, on the supposition that the increase in weight would continue regularly. Repeat the same process, using the pan (24) instead of the panel (23), obtaining a small "blank" increase in weight. The same blank may be used for successive measurements with proper precautions. The number of milligrams increase for the panel, minus the blank, expresses the weight of moisture that has diffused through the panel in 24 hours and is thus its "actual permeability." Since duplicate panels will rarely have the same weight of paint and since a thicker coat will usually show a smaller perviousness, the actual results were generally multiplied by one-tenth the weight of each coat, to obtain results on the basis of weight of film or the "weight permeability." One-tenth was selected because the average weight of coat was about 10 g.

(B) THE METHOD OF ARTIFICIAL EXPOSURE.

Location of the Apparatus.—Fig. 7, Plate IV, shows vertical sectional and horizontal sectional views of the apparatus as used during the winter of 1912-13 in the joint laboratory of

the Patton Paint Co., the Pitcairn Varnish Co., and the Corona Chemical Co., Milwaukee, Wis. The author wishes here to express to these firms gratitude for many courtesies both during the construction and during the operation of the apparatus.

Construction of the Testing Tank.—Fig. 7 shows the cylindrical testing tank (1), 7 ft. in external diameter and 4 ft. in height; the lighting cover (2); and the spraying cover (3). The tank is lined on its inner side and bottom with galvanized sheet iron (4), the bottom having a slope of 11 in. from the sides to the center (6), where a $1\frac{1}{2}$ -in. pipe (7), connected to a rubber hose (8), carries off air or water, as the case may be through the drain (9). The tank is on casters (10) and can thus be easily rolled into position under the spraying cover (3), as well as under the lighting cover (2), in which position it stands in the figure. On the outside the tank is lined with 3-in. oak flooring (18), $\frac{7}{8}$ in. thick, leaving an air space of $2\frac{1}{8}$ in. on the sides and a larger space on the bottom, as shown. The whole tank is enameled thoroughly, being colored white on the inside. One-inch pure gum rubber hose (42) is tacked on the top of the sides, so as to make an air-tight seal for a cover, when the latter rests in position. Tiers of galvanized iron strips (11) were riveted and soldered to the sides and bottom of the tank, as shown in Fig 7, during the construction of the metallic lining. These strips are each $\frac{1}{2}$ in. wide and project $1\frac{1}{2}$ in. from the lining. They are designed to hold the tiers of painted panels (12, 13, 14, 15, 16) in correct position touching the lining and horizontal for the same tier, designated by the same numeral. For example, all panels of any comparative set go in tier No. 12 or tier No. 13, not part in No. 12 and part in No. 13. There is room for 110 panels altogether in the tank, but the tiers on the bottom are only used in special cases. Three or four strips are required to hold each panel.

Construction of the Spraying Cover.—Both the spraying (3) and the lighting (2) covers are constructed alike and of similar materials to those of the tank. They are 7 ft. in diameter and lined on the under side with galvanized sheet iron (17). On the upper side they have 3-in. oak flooring (18), with a $2\frac{1}{8}$ -in. thickness of air space. They are also enameled, and the light-

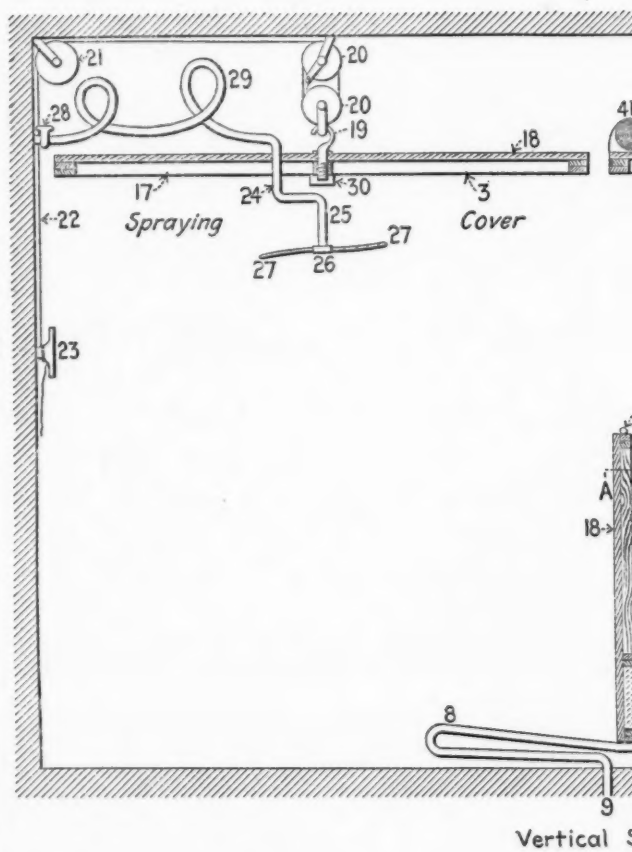
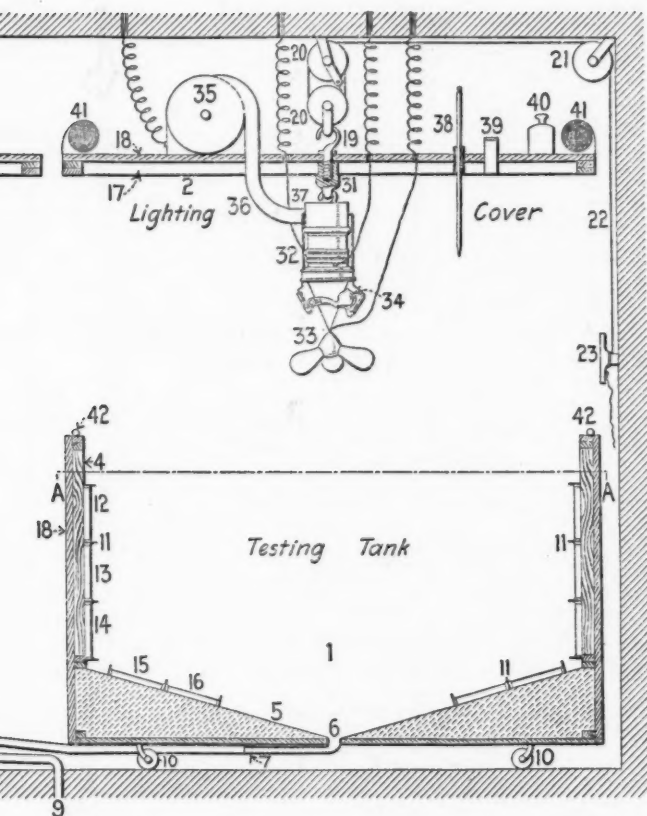
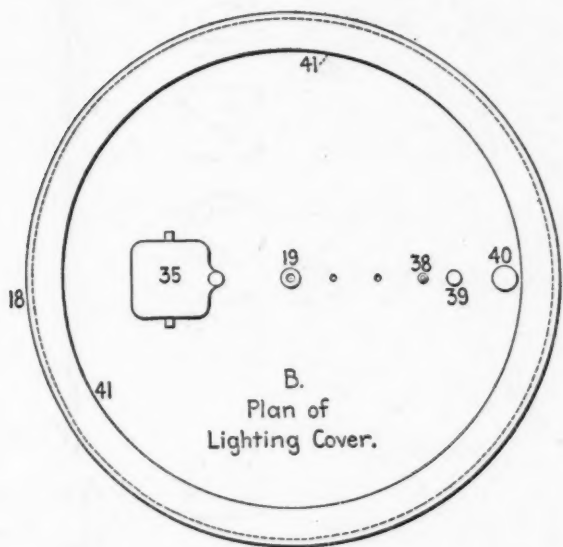
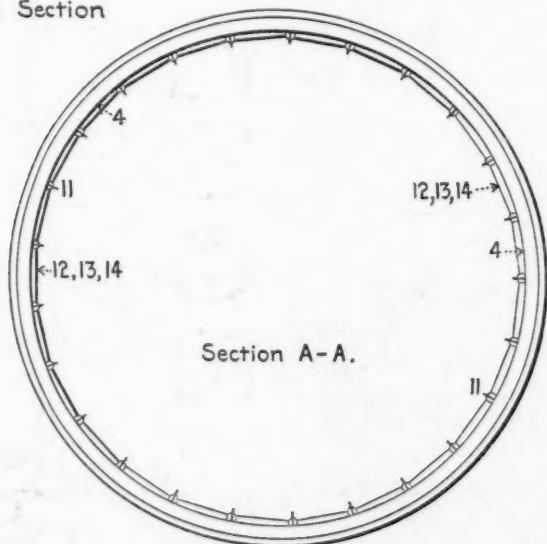


FIG. 7.—Testing Tank

PLATE IV.
 PROC. AM. SOC. TEST. MATS.
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Vertical Section



Testing Tank and the Two Covers.

ing cover is coated white on the metallic side. Both covers have hooks bolted to their centers, from which they are suspended to the ceiling by means of a pair of double pulleys (20). A third pulley (21) carries the rope (22) to the cleat (23), from which point the cover can be raised or lowered to any desired level. Seven inches from the center of the spraying cover (3), a 1-in. pipe (24) enters from above and bends around so as to point down (25) perpendicularly from the center. This pipe ends 1 ft. below the cover in a whirling garden spray (26), having four spokes (27). Each spoke is set at a different angle, so that when the cover is down on the tank and the spray is in operation, each tier of panels will receive roughly its share of impinging water. The pipe (24) is connected to the water faucet (28) by means of the hose (29), and this faucet is always opened to the same angle when using the spray.

Construction of the Lighting Cover.—Instead of the nut (30) which holds the hook (19) of the spraying cover, the arrangement (31) is used, so that the Cooper-Hewitt mercury-vapor quartz-tube lamp (32) may be suspended from the center of the cover. The lamp used was type Y (110 volts), affording 1000 candle power. The globe to the lamp was not used, but the two chains for holding the latter were applied instead to suspending a cluster of three Mazda tungsten lamps (33), 8 in. below the quartz tube (34) and 3 ft. below the cover. The Mazda lamps were each 250 watts, 110 volts, 220 candle power. The color of the above combination seemed to match sunlight fairly well. The electrically driven fan (35) blows air through the 2-in. rubber hose (36) into the cup (37) fitted over the casing of the Cooper-Hewitt lamp, in such a direction as to distribute the ventilation as nearly as possible out from the center, at the same time cooling the auxiliary of the lamp while it is in operation in the tank. The equipment of the lighting cover is completed by the thermometer (38), the peep-hole (39), and the counterbalancing weight (40). The rolls of duck cloth (41) are for another purpose. With the lighting cover and tank in position shown in Fig. 7, the rolls (41) may be let down around the sides of the tank, and by means of a large hole in the cloth opposite a window, may be quickly attached to a similar roll brought out from the window, in such a manner that the warm

air of the tank may flow out and the cold outer air freely flow into the tank, without appreciably affecting the temperature of the room.

The Schedule of Artificial Exposure.—(a) In order to make a set of comparative tests, such as is to be found recorded on any one of the charts of the illustrations to follow, each coat is spread on the panels the same day as rapidly as possible, and when the successive coats are sufficiently dry, the panels are all placed on separate units of the testing device in the constant-temperature room for a measurement of their initial permeabilities. Next, they are placed on the same tier of the testing tank at the same time for a given number of days of artificial exposure; then returned to the constant-temperature room, allowed to dry from 12 to 24 hours, and placed on the testing devices for a second measurement of their permeabilities, and so on. In making the permeability measurements, one cover of the testing devices after another is removed for putting on the weighed dishes, and replaced. The same succession for the various panels and weighed dishes and the same rapidity in making the measurement changes are always observed.

(b) The circumference of the lighting cover had been divided into 6 equal segments and the middle of each marked for a particular day of the week. A vertical mark was also made on the upper portion of the outside of the tank. The actual schedule observed for the artificial exposure was as follows: For 6 days of the week, at 7 a. m., the lighting cover (2) was lowered on the tank in such a way as to have its mark for that particular day of the week just above the mark on the tank. Both the mercury and tungsten lamps were switched on and when the temperature of the tank had reached 130° F., the fan (35) was started and maintained at such a speed as to keep the temperature at 130° F. It usually required 2 hours to reach the above temperature. Occasionally the temperature reached 140° F. and frequently was maintained at 120° F.

(c) At 6 p. m. the lights and fan were switched off and the lighting cover raised to the ceiling. The tank was then rolled into position under the spraying cover (3) and the latter was lowered upon the former. The water was then turned on, a tolerably active spray being maintained. At 11 p. m., the

water was cut off, and the spraying cover raised to the ceiling. The tank was moved to its position under the lighting cover, close to a window, the cloth (41) unrolled and attached to the tank and to the cloth from the open window. The next morning at 7 a. m., the cloths were rolled up, as shown in Fig. 7, and another week-day schedule commenced.

(d) At 6 p. m. Saturdays, the spray was turned on as usual and maintained until 6 a. m. the next day, when the water was turned off, the cover raised to the ceiling, and the tank allowed to dry out until 7 a. m. Monday morning. The above schedule for 7 successive days was not followed slavishly, since in any event all panels of any comparative set were treated exactly alike, but it was followed as closely as convenient. The panels were always removed from or replaced in the tank the first thing in the morning. In the final interval of tests given in this paper and running more than 60 days, the temperature of the tank was maintained at 100° F. during the day, and from 11 p. m. to 7 a. m. no cold air was let in, but the spray cover with water turned off was simply raised to the ceiling so that the tank could dry out. It should be stated that in tabulating results, only those days were counted during which the panels had been exposed to artificial light.

(C) THE PRACTICAL TESTING OF COMMERCIAL COVERINGS.

The main portion of the preliminary report of this investigation was devoted to actual results on paints and varnishes. None of the diagrams there given will be shown here, except where for brevity's sake part of any series of tests had been omitted, or where further results have been obtained on any set of panels since the first paper was written. The latter work was performed in the absence of the author by the chemists of the Patton Paint Co., to which firm the author acknowledges obligations for continuing and completing the tests. The last intervals on pigments, oils, and driers, metallic coverings, and composite whites were carried through also by them. Only the additional data for the curves are tabulated in this report.

For all panels, except those of the tests on metallic coverings, six red leads, and cement coatings, the porous surface used consisted of Max Dreverhoff's hardened filter paper, No. 496. For

the first two above mentioned, steel-wire cloth, 80 mesh, was used. In the latter cases, naturally, the first coat had to be spread on both sides, in order to fill all the meshes well. Some difficulty was encountered in preventing the mercury of the grooves from attacking the solder of the panels, and occasionally this caused a disagreement of duplicate tests. Throughout this

TABLE I.—ADDITIONAL RESULTS ON IMPLEMENT PAINTS (FIGS. 8, 9).

IMPLEMENT VERMILIONS, FIRST SET													
Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.	
				Actual	By Weight			Actual	By Weight			Actual	By Weight
FM ₁	6.24	1	0	1579	1008	2	3	2078	1297	3	5½	3193	1992
FM ₂	5.19	1	0	1381	717	2	3	2344	1217	3	5½	3411	1770
Average.....		1	0	1480	862	2	3	2211	1257	3	5½	3302	1881
FM ₁		4	4½	4521	2821
FM ₂		4	4½	3927	2038
Average.....		4	4½	4224	2429

IMPLEMENT VERMILIONS, SECOND SET.													
FN ₁	4.26	1	0	1845	786	2	3	2024	862	3	5½	2647	1128
FN ₂	3.80	1	0	1715	652	2	3	2210	840	3	5½	4607	1751
Average.....		1	0	1780	719	2	3	2117	851	3	5½	3627	1439
FN ₁		4	4½	3984	1697
FN ₂		4	4½	6792	2581
Average.....		4	4½	5388	2139
FO ₁	3.47	1	0	2048	711	2	3	1822	643	3	5½	2690	1027
FO ₂	3.15	1	0	1899	598	2	3	1798	566	3	5½	2562	816
Average.....		1	0	1973	654	2	3	1810	604	3	5½	2626	921
FO ₁		4	4½	5373	1865
FO ₂		4	4½	4236	1334
Average.....		4	4½	4804	1599
FP ₁	2.62	1	0	2099	550	2	3	1858	487	3	5½	1990	521
FP ₂	3.00	1	0	2037	611	2	3	1791	537	3	5½	1972	592
Average.....		1	0	2068	580	2	3	1824	512	3	5½	1981	556
FP ₁		4	4½	2277	597
FP ₂		4	4½	2355	707
Average.....		4	4½	2316	652

BRUSHING GREENS.													
FR.....	3.57	1	0	2282	815	2	3	2851	1018	3	5½	3983	1422
FR.....		4	4½	5289	1888
FS.....	2.04	1	0	2438	717	2	3	2636	775	3	5½	2720	800
FS.....		4	4½	2682	789	5	7	3150	926

paper composition or analyses will be given as far and in the same way as the manufacturers have furnished them. In this way it is hoped that they and the public will have a clearer estimate of the value of the diagrams shown.

Implement Paints.—In Fig. 7 of the preliminary report previously referred to, four implement paints were plotted.

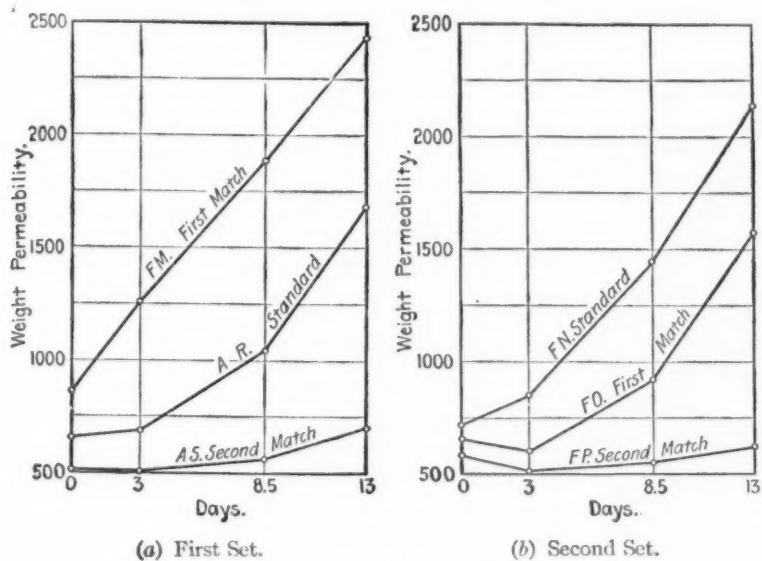


FIG. 8.—Implement Vermilions.

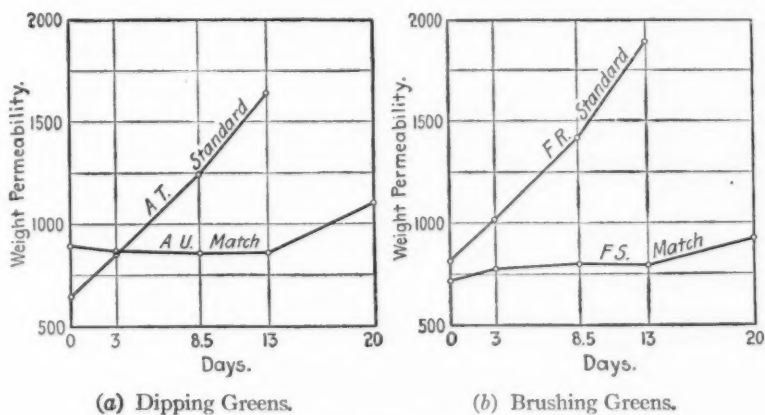


FIG. 9.—Greens.

This diagram is now expanded into Figs. 8 (a) and (b) and 9 (a) and (b), the curves reported twice being identified by the same letters, as indeed will be the rule throughout this paper. Table I accompanies the figures. These ten coatings were all put on exactly as if on a wagon or a plow. First came the pink primer, consisting of lithopone, iron oxide, and a little zinc oxide, ground in linseed oil, with 10 per cent of japan, and finally thinned to dipping consistency with benzine. This was the same for all panels, with as far as possible the same weight of primer on each. Next came the particular paste for each set of duplicates, thinned down with benzine. Finally the same super-coat of varnish, identical with *BK* of Fig. 16, was spread on all to the same thickness as far as possible, the liquid being thinned as above. The pastes had the following compositions:

CLASS.	COMPOSITION.	PER CENT.
<i>AR</i> ¹	Orange mineral.....	51.27
	Barium sulfate.....	38.50
	Calcium carbonate.....	10.23
<i>FM</i> and <i>AS</i>	Para.....	2.79
	Orange mineral.....	43.4
	Barium sulfate.....	40.6
	Linseed oil.....	13.3
<i>FO</i>	Para on calcium carbonate.....	57.8
	Lithopone.....	14.5
	Organic matter.....	1.2
	Linseed oil.....	21.7
	Drier.....	4.8
<i>FP</i>	Para on calcium carbonate.....	53.8
	Lithopone.....	15.4
	American vermilion.....	7.8
	Linseed oil.....	19.2
	Drier.....	3.8
<i>AU</i>	Lithopone.....	2.6
	Commercial green.....	63.2
	Drop black.....	2.6
	Whiting.....	10.5
	Linseed oil.....	15.8
	Benzine.....	5.3

¹ Vehicle not reported.

CLASS.	COMPOSITION.	PER CENT.
<i>FS</i>	Commercial green.....	62.3
	Drop black.....	5.2
	Whiting.....	10.4
	Lead chromate.....	1.3
	Linseed oil.....	15.6
	Benzine.....	5.2

In the curves for these implement paints, "standard" refers to the pastes that were in actual use by large manufacturers. The expressions "first match" and "second match" refer to successive attempts to improve on the materials in use. In Fig. 8 (*a*), the first match was evidently a failure, but the second a vast improvement. In Fig. 8 (*b*), both matches are better than the standard, but the first is not much so. In Fig. 9 (*a*), the match is so much better than the standard that its test was run 20 days before it broke down (or rose in permeability). The same was true of Fig. 9 (*b*). In fact, in all four of these figures, the word standard is a misnomer, in no case representing a paste that would hold up 3 days.

There ought to be no doubt, at least in the case of these implement paints, as to the meaning of these charts. In no event should a manufacturer use a coating like *AR*, *FN*, *AT*, or *FR*. Their very appearance even before the end of the test betrayed them. It would have been difficult to show this by photograph, on account of the colors involved, but the checks, large and small, that covered them, left no doubt in the minds of all who saw them as to which had the higher and which the lower permeability. Photographs published with the preliminary report, one of which is here reproduced as Fig. 21, are illustrative of the general appearance of all films, tested by this process, that rose in permeability (or broke down) at some point during exposure. High permeability and unsatisfactory appearance always went hand in hand.

It is difficult to explain the remarkable differences between the conduct of these implement paints. That *FM* and *AS* should have the same composition and yet act so differently is of course out of the question. The duplicate tests on each agree quite well, considering the possible variations in undercoat and supercoat, so that it is a fair assumption that there

was a difference in their composition; probably more oil was added to *AS* than to *FM*.

Pigments, Oils, and Driers.—If tests are made upon commercial paints, one cannot, as shown in the foregoing, avoid several sources of error, such as differences in the oils, driers, quality of the pigments, and intimacy of incorporation. With a view to neutralizing such errors, 14 formulas were arranged so as to give the greatest number of comparisons and after the corresponding liquids were mixed, panels were prepared and tested simultaneously for many weeks. No duplicates were made for the least important formulas. The same batch of material was used for all the liquids where the formula called for it. The lead and manganese linoleates were obtained by precipitation and subsequent purification.

The constituents of the formulas below are given in parts by weight. *CT* has one-half the lead of *CR* and one-half the zinc of *CS*. In *CU*, one-fifth of the lead and of the zinc of *CT* is replaced by silica, with 0.47 part additional. The first eight formulas contain the lead and manganese linoleates in the proportion of 0.3 and 0.22 part, or a total of 0.52 part to 40 parts of oil. This was figured as a standard proportion of 4 to 1 parts for these metals in the elementary state. In *DA* and *DB* there is only metallic drier and twice as much as for that metal in the standard. *DC* was calculated so as to have the same total quantity of elementary lead and manganese as in the standard, but in the proportion of 1 to 1. The same was true of *DG* and *DH* japans as regards total weight of elementary lead plus manganese. The pigments were ground to paste form before mixing with driers and remaining oil, and the assumption below as to composition was correct to within a small percentage of error.

CLASS.	COMPOSITION.	PARTS BY WEIGHT.
<i>CR</i>	Corroded lead.....	70.0
	Lead and manganese linoleates.....	0.52
	Linseed oil.....	40.0
<i>CS</i>	Zinc white.....	35.0
	Lead and manganese linoleates.....	0.52
	Linseed oil.....	40.0

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CLASS.	COMPOSITION.	PARTS BY WEIGHT.
<i>CT</i>	Corroded lead.....	35.0
	Zinc white.....	17.5
	Linseed oil.....	40.0
	Lead and manganese linoleates.....	0.52
<i>CU</i>	Corroded lead.....	28.0
	Zinc white.....	14.0
	Silica.....	10.97
	Linseed oil.....	40.0
	Lead and manganese linoleates.....	0.52
<i>CV</i>	Asbestine.....	39.0
	Linseed oil.....	40.0
	Lead and manganese linoleates.....	0.52
<i>DA</i>	Asbestine.....	39.0
	Linseed oil.....	40.0
	Lead linoleate.....	0.6
<i>DB</i>	Asbestine.....	39.0
	Linseed oil.....	40.0
	Manganese linoleate.....	0.44
<i>DC</i>	Asbestine.....	39.0
	Linseed oil.....	40.0
	Lead linoleate.....	0.12
	Manganese linoleate.....	0.39
<i>DE</i>	Asbestine.....	39.0
	Linseed oil.....	40.0
	<i>DG</i> japan.....	2.7
<i>DF</i>	Asbestine.....	39.0
	Linseed Oil.....	40.0
	<i>DH</i> japan.....	8.5
<i>DG</i>	Kauri.....	5.3
	Rosin.....	2.7
	Linseed oil.....	16.5
	Driers.....	4.8
	Benzine.....	70.7
<i>DH</i>	Rosin.....	21.4
	Linseed oil.....	11.8
	Driers.....	3.5
	Benzine.....	63.3

The results on testing out these formulas are given below.
The comparisons will be made by three sets of curves. (See also Table II.)

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TABLE II.—RESULTS ON PIGMENTS, OILS, AND DRIERS (FIGS. 10, 11, 12).

Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.	
				Actual	By Weight			Actual	By Weight			Actual	By Weight
CR ₁	11.89	1	0	2900	3448	2	8	2487	2957	3	27	2107	2505
CR ₂	13.39	1	0	3146	4212	2	8	2668	3542	3	27	2469	3306
Average.....		1	0	3023	3834	2	8	2577	3264	3	27	2288	2905
CR ₁		4	48	1842	2190	5	24	1908	2269				
CR ₂		4	48	1888	2524	5	24	1980	2645				
Average.....		4	48	1865	2357	5	24	1936	2457				
CS ₁	9.71	1	0	2364	2295	2	8	1832	1779	3	27	1595	1549
CS ₂	9.12	1	0	2450	2234	2	8	1907	1700	3	27	1700	1560
Average.....		1	0	2407	2265	2	8	1869	1739	3	27	1653	1554
CS ₁		4	48	1571	1527	5	24	1686	1636				
CS ₂		4	48	1591	1450	5	24	1697	1550				
Average.....		4	48	1581	1488	5	24	1691	1593				
CT ₁	10.87	1	0	2594	2820	2	8	1935	2103	3	27	1747	1899
CT ₂	10.86	1	0	2372	2576	2	8	1772	1924	3	27	1597	1734
Average.....		1	0	2483	2698	2	8	1853	2013	3	27	1672	1816
CT ₁		4	48	1528	1664	5	24	1697	1845				
CT ₂		4	48	1463	1592	5	24	1609	1750				
Average.....		4	48	1495	1628	5	24	1644	1797				
CU ₁	10.67	1	0	1945	2075	2	8	1422	1517	3	27	1297	1385
CU ₂	10.72	1	0	2067	2216	2	8	1483	1590	3	27	1311	1405
Average.....		1	0	2006	2146	2	8	1452	1553	3	27	1304	1395
CU ₁		4	48	1206	1287	5	24	1259	1343				
CU ₂		4	48	1193	1281	5	24	1317	1413				
Average.....		4	48	1200	1284	5	24	1288	1378				
CV ₁	13.87	1	0	1595	2213	2	8	1308	1814	3	27	1244	1725
CV ₂	12.23	1	0	1895	2318	2	8	1644	2010	3	27	1535	1876
Average.....		1	0	1745	2266	2	8	1476	1912	3	27	1389	1800
CV ₁		4	48	1169	1620	5	24	1212	1682				
CV ₂		4	48	1389	1699	5	24	1359	1662				
Average.....		4	48	1279	1659	5	24	1285	1673				
CW ₁	22.22	1	0	1365	3033	2	8	1048	2275	3	27	737	1638
CW ₂	21.72	1	0	1357	2947	2	8	978	2124	3	27	929	2038
Average.....		1	0	1361	2990	2	8	1013	2199	3	27	833	1828
CW ₁		4	48	755	1678	5	24	892	1980				
CW ₂		4	48	776	1686	5	24	1002	2180				
Average.....		4	48	765	1682	5	24	947	2080				
CX.....	12.25	1	0	2473	3029	2	8	2121	2598	3	27	1651	2022
CX.....		4	48	1488	1822	5	24	1455	1785				
CY.....	13.26	1	0	2592	3437	2	8	2148	2848	3	27	1785	2367
CY.....		4	48	1398	1855	5	24	1533	2032				
DA ₁	9.78	1	0	2485	2430	2	8	2059	2014	3	27	1735	1697
DA ₂	10.53	1	0	2198	2315	2	8	1821	1917	3	27	1560	1643
Average.....		1	0	2341	2373	2	8	1940	1965	3	27	1647	1670
DA ₁		4	48	1564	1530	5	24	1544	1510				
DA ₂		4	48	1445	1522	5	24	1420	1499				
Average.....		4	48	1504	1526	5	24	1482	1505				
DB ₁	8.72	1	0	2339	2040	2	8	2093	1828	3	27	1963	1713
DB ₂	8.66	1	0	2446	2163	2	8	2025	1754	3	27	1801	1560
Average.....		1	0	2389	2102	2	8	2059	1791	3	27	1882	1636
DB ₁		4	48	1514	1320	5	24						
DB ₂		4	48	1472	1276	5	24	1192	1032				
Average.....		4	48	1493	1298	5	24		(1049)				
DC.....	9.38	1	0	2280	2140	2	8	1876	1760	3	27	1823	1710
DC.....	10.03	1	0	1858	1864	2	8	1681	1686	3	27	1555	1560
Average.....		1	0	2069	2002	2	8	1778	1723	3	27	1689	1635
DC.....		4	48	1466	1376	5	24	1291	1212				
DC.....		4	48	1381	1384	5	24	1281	1285				
Average.....		4	48	1423	1380	5	24	1286	1249				
DE.....	10.90	1	0	2221	2421	2	8	1876	2045	3	27	1728	1884
DE.....	11.27	1	0	2159	2433	2	8	1746	1968	3	27	1544	1740
Average.....		1	0	2190	2427	2	8	1811	2006	3	27	1636	1812
DE.....		4	48	1448	1576	5	24	1362	1485				
DE.....		4	48	1416	1596	5	24	1282	1443				
Average.....		4	48	1432	1587	5	24	1322	1464				
DF.....	10.12	1	0	2209	2236	2	8	1750	1780	3	27	1581	1600
DF.....		4	48	1407	1425	5	24	1261	1278				
DP.....	6.94	1	0	193	134	2	10	12153	8434	3	27		

Four Pigments.—In Fig. 10, the high value for CR is simply due to the great weight of the lead film. The comparison lies in the slopes of the curves rather than in their position in reference to the ordinate. There is not very much difference developed between the coatings, all except the asbestine increasing in permeability during the last interval of mild exposure. Probably the kind of exposure was not adapted to bringing out best the inherent differences between these pigments.

Four Oils.—The results on four oils are more satisfactory,

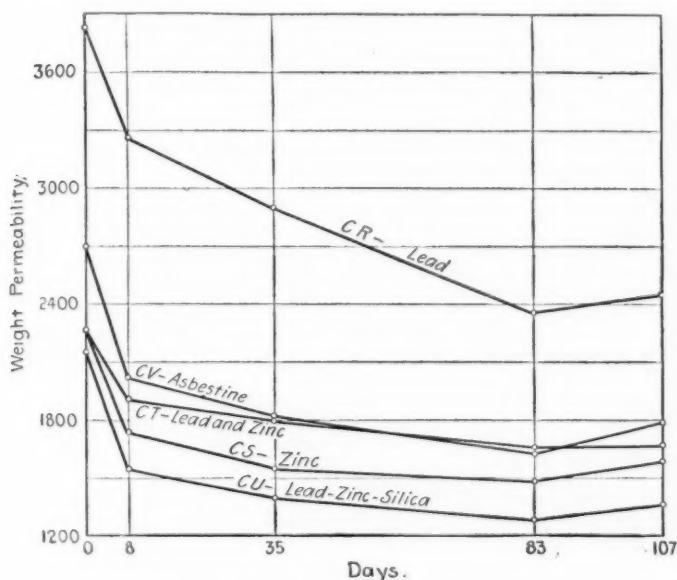


FIG. 10.—Four Pigments.

as illustrated in Fig. 11. The asphaltum of the metallic coverings is here reproduced for the sake of contrast. These oils had not been analyzed fully and the menhaden especially may not have been up to the standard. Certainly it and the Chinese wood oil go to pieces on the last interval, the latter being a much thicker coating than the others. It is interesting to note that linseed oil holds up the best, though soya bean oil is a good second. These two illustrations seem to indicate that differences between the oils—the vehicle of the liquid and the binder

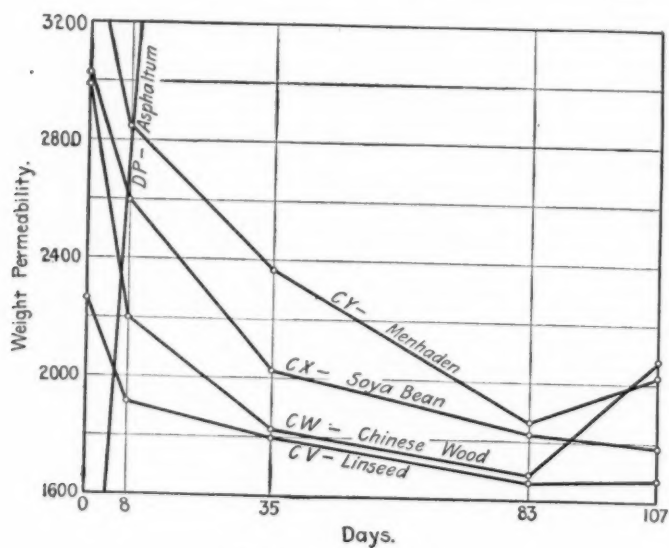


FIG. 11.—Four Oils.

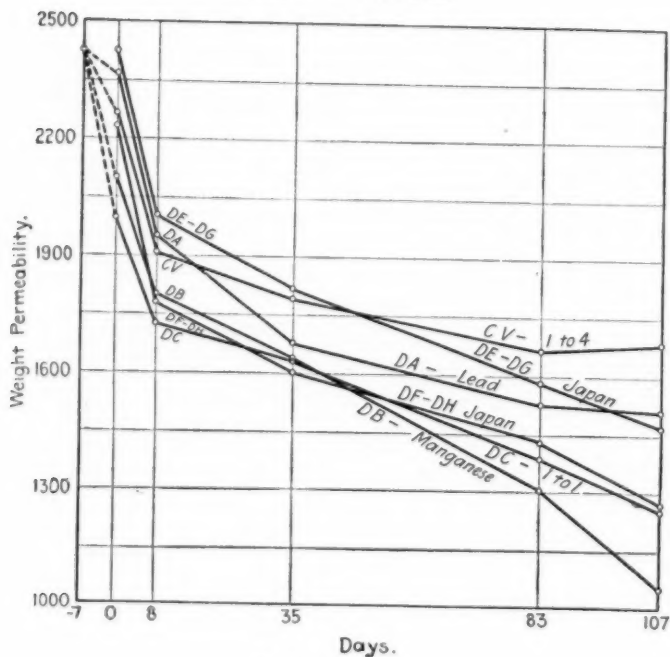


FIG. 12.—Driers.

of the dried paint—are more easily developed than between the pigments in the same.

Driers.—Fig. 12 predicts that the differences in the setting-up of coatings as well as the later effect of the drier can be recorded in the changes in permeability of the exposed panels, though the standard methods for the former are satisfactory. Since *CV*, *DA*, *DB*, and *DC* have the same composition except for the trace of drier, it is fair to presume, as will be shown later, that all of the panels of these possessed about the same weight permeability when fresh. This could have been measured. Assuming it for purposes of illustration to have been 2427, not less than that of *DE* of practically the same com-

TABLE III.—RESULTS ON METALLIC COVERINGS (FIG. 13).

Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.	
				Actual	By Weight			Actual	By Weight			Actual	By Weight
DM.....	15.92	1	0	758	1207	2	19	480	764	3	49½	338	538
DN ₁	12.18	1	0	827	1007	2	19	549	669	3	49½	418	509
DN ₂	11.79	1	0	756	891	2	19	582	686	3	49½	368	434
Average.....		1	0	791	949	2	19	565	677	3	49½	393	472
DN ₁		4	46	495	604
DN ₂		4	46	409	482
Average.....		4	46	452	543
DO.....	27.96	1	0	1230	3439	2	19	977	2732	3	49½	550	1538
DP.....	6.94	1	0	453	1267
DQ.....	18.58	1	0	193	134	2	19	12153	8434
DL.....	8.77	1	0	741	1377	2	19	321	596	3	49½	425	790
DM.....		4	46	1140	1000	794	696	3	49½	459	403
DM.....				461	734

position, we note that the 1-to-1 proposition dries most thoroughly during the week that elapsed from the first coat to the first measurement, and that manganese comes next in setting-up power. Dotted lines on Fig. 12 show the trend of these probable pre-measurement changes.

There is a very noticeable contrast in Fig. 12 between the standard 1-to-4 drier and the straight manganese, as would be expected. The manganese keeps on going down in permeability, while the 1-to-4 drier at the end of the test is over 600 points above it, though all are supposed to have started with the same perviousness. Straight lead is about the same as the 1-to-4 drier, showing the influence of the lead in the mixture,

while the 1-to-1 is about the same as the straight manganese, showing the influence of the latter constituent. The 1-to-4 means nearly an atom for atom mixture, so that there must be an excess of manganese atoms to have the characteristic drying effect of pure manganese. In general this series of tests encourages the belief that this method of testing will result in a more scientific differentiation between the driers.

Metallic Coverings.—Let us now consider metallic coverings,

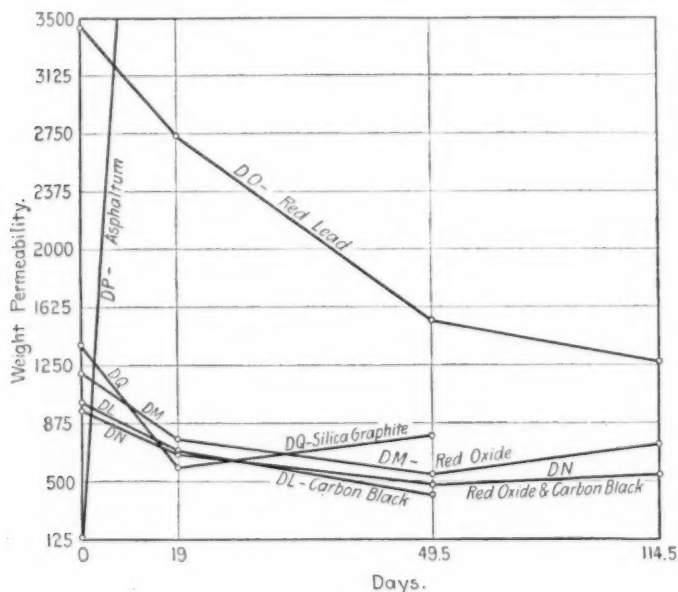


FIG. 13.—Metallic Coverings.

of which Table III and Fig. 13 are apt illustrations. Since the object of the investigation at the present stage was to demonstrate the possibilities of a new method over a wide range rather than to obtain accurate results over a small range, it was found impracticable, on account of the limited space in the testing tank, to attempt to secure checks on all these measurements and on others plotted later, though indeed each measurement on a panel acts as a check on the preceding and succeeding ones. It was also found impossible to make a

measurement during the last interval on *DQ* and *DL*. The compositions are recorded below:

CLASS.	COMPOSITION.	PER CENT.
<i>DM</i>	Iron oxide.....	25.8
	Silica and silicates.....	17.2
	Basic lead chromate.....	8.6
	Linseed oil.....	43.6
	Turpentine and drier.....	4.8
<i>DP</i>	Asphaltum.....	28.0
	Linseed oil.....	10.8
	Driers.....	2.8
	Benzine.....	58.4
<i>DL</i>	Carbon black.....	7.4
	Sublimed lead.....	2.3
	Linseed oil.....	82.4
	Driers and japan.....	7.9
		PARTS.
<i>DO</i>	Red lead.....	20 lb.
	Linseed oil.....	$\frac{5}{8}$ gal.
	Turpentine substitute.....	$\frac{1}{2}$ pint.
	Japan drier.....	$\frac{1}{2}$ pint.
<i>DO</i>	Graphitic carbon.....	2 lb.
	Silica.....	2 lb.
	Boiled linseed oil.....	2 lb.
	Benzine.....	$\frac{3}{8}$ gal.
	Japan.....	$\frac{1}{64}$ gal.

In Fig. 13, the high value for the red lead, as for the white previously considered, is due to its high specific gravity. Unfortunately two of the panels could not be tested during the last interval. There are nevertheless some interesting contrasts. *DQ*, the silica graphite, which name refers to no special commercial product, broke down during the second interval. Panel *DN* consisted of the first coat of the carbon black and the last coat of the red oxide. The curve for this—a commercial proposition—shows up the best of all, except that of the red lead. *DP* went to pieces with extreme rapidity, as Fig. 13 shows. When removed from the testing tank, holes were visible in the meshes of the wire cloth to a large extent. On the whole this set of tests is encouraging.

Exterior Enamels.—Fig. 14 is a reproduction of Fig. 20 of the preliminary report, except that the additional data (Table IV) and extension of curve for *ED* up to the point that it

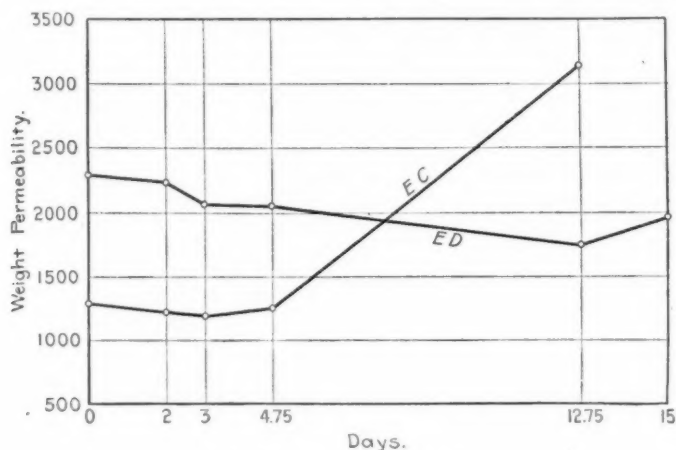


FIG. 14.—Exterior Enamels.

broke down is given. The striking divergence between these two exterior enamels must again be emphasized. Certainly *ED* is the far preferable one. The results, as is to be seen, were

TABLE IV.—ADDITIONAL DATA FOR EXTERIOR ENAMELS (FIG. 14).

Class.	Measurement No.	Interval, days.	Permeability.	
			Actual.	By Weight.
ED ₁	6	15	2343	2000
ED ₂	6	15	2260	1965
Average.....	6	15	2306	1962

obtained in only 15 days. The appearance of *EC* at the end of the experiment was poor, the surfaces of the duplicates being covered with checks. The composition was reported as follows:

CLASS.	COMPOSITION.	PER CENT.
<i>EC</i>	Zinc oxide.....	35.6
	Varnish.....	64.4
<i>ED</i>	No report.	

Cement Coatings.—Fig. 15 (see Table V) is likewise a reproduction of the former Fig. 12, with another panel of coating *CH*, termed *CI*, and still another, *CJ*, added, in these cases with a 29-day exposure. This is to show that not all cement coatings

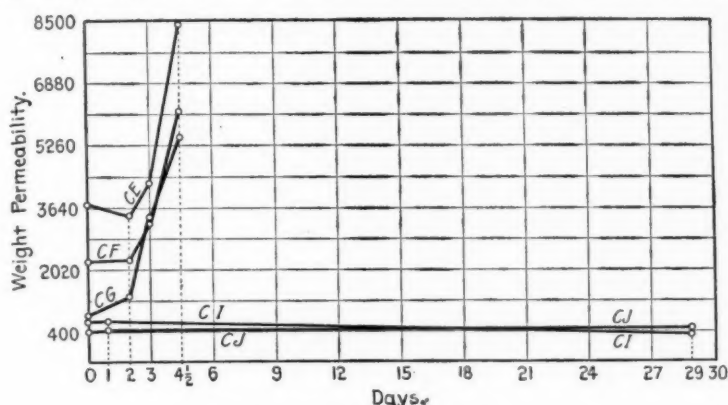


FIG. 15.—Cement Coatings.

go to pieces so rapidly as *CE*, *CF*, and *CG*. The constituents of these coatings were furnished as below:

CLASS.	COMPOSITION.	PER CENT.
<i>CE</i> ¹	Zinc oxide.....	35.13
	Sublimed lead.....	35.13
	China clay.....	11.12
	Asbestine.....	18.62
<i>CF</i>	Zinc oxide.....	26.4
	Magnesium silicate.....	17.5
	Barium sulfate.....	17.5
	Linseed oil.....	2.6
	Flatting liquid.....	36.0
<i>CG</i>	Zinc oxide.....	26.5
	Lithopone.....	26.5
	Special floor paint varnish.....	26.5
	Treated Chinese wood oil.....	10.5
	Petroleum spirits, naphtha.....	10.0
<i>CI</i>	Zinc oxide.....	18.9
	Magnesium silicate.....	12.5
	Barium sulfate.....	12.5
	Flatting liquid.....	56.1

¹ Vehicle reported as a varnish.

CLASS.	COMPOSITION.	PER CENT.
<i>CJ</i>	Zinc oxide.....	32.3
	White lead.....	10.8
	Flatting liquid.....	56.9

Among the many possibilities in imitating actual service conditions by the process of testing here being presented is the use of the same surface as the liquid is designed to cover. There

TABLE V.—ADDITIONAL DATA ON CEMENT COATINGS (FIG. 15).

Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.	
				Actual.	By Weight			Actual.	By Weight			Actual.	By Weight
<i>Ch</i>	9.50	1	0	721	685	2	1	838	797	3	28	1145	1088
<i>Cl</i>	9.12	1	0	740	675	2	1	697	635	3	28	1018	956
Average.....		1	0	730	680	2	1	767	716	3	28	1096	1022
<i>Cj</i>	5.59	1	0	587	496	2	1	875	488	3	28	951	1091
<i>Cl</i>	6.78	1	0	778	537	2	1	778	527	3	28	1600	1088
Average.....		1	0	832	511	2	1	827	508	3	28	1775	1089

TABLE VI.—ADDITIONAL DATA FOR TWELVE VARNISHES (FIG. 16).

Class.	Measurement No.	Interval, days.	Actual Permeability.	Measurement No.	Interval, days.	Actual Permeability.
<i>BN</i> ₁	5	13	7852
<i>BN</i> ₂	5	13	6052
Average.....	5	13	6952
<i>BM</i> ₁	5	13	9584
<i>BM</i> ₂	5	13	7360
Average.....	5	13	8412
<i>BL</i> ₁	5	13	4824	6	15	5 070
<i>BL</i> ₂	5	13	5180	6	15	5 382
Average.....	5	13	5002	6	15	5 226
<i>FL</i> ₁	1	0	1486	2	5½	12 674
<i>FL</i> ₂	1	0	1621	2	5½	12 273
Average.....	1	0	1553	2	5½	12 473

is room for much improvement along this line, which portion of the research will be prosecuted further, but in the case of these cement coatings, almost the ideal surface was used. It was $\frac{3}{8}$ in. thick and was composed of 1 part of Portland cement to 2 parts of sand. Fig. 15 shows how much improvement is needed in the cement protectives now selling on the market and indicates the need for a complete investigation of this line of coverings.

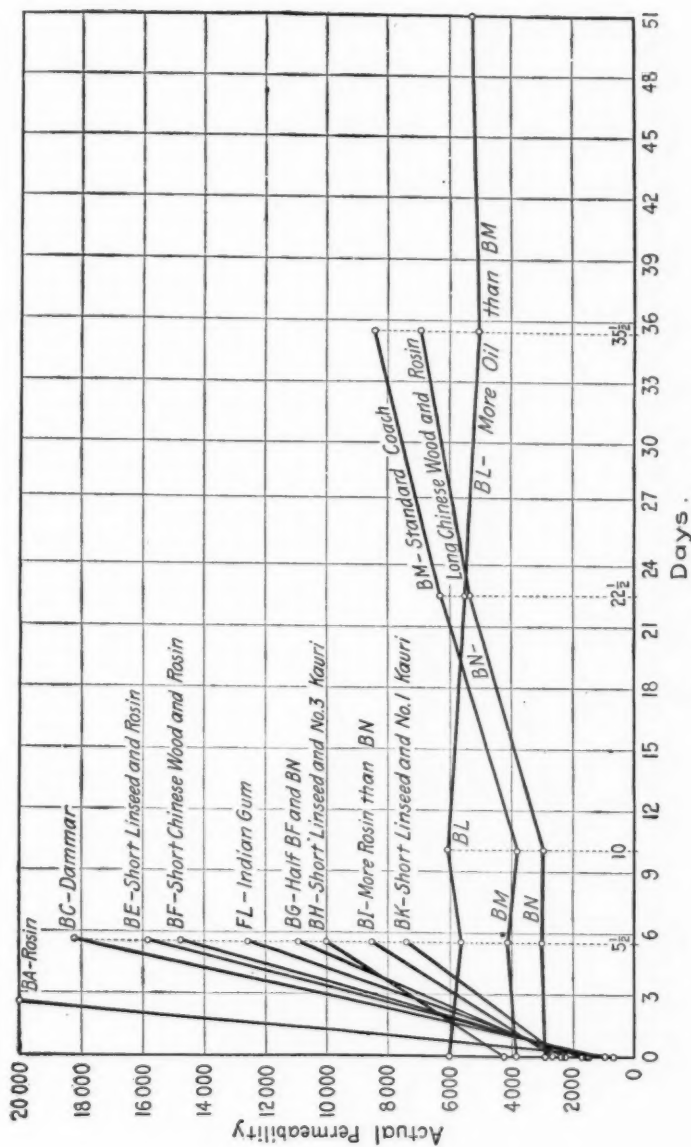


FIG. 16.—Twelve Varnishes.

Twelve Varnishes.—Fig. 16, with one additional varnish and with *BM* tested until its curve has started to rise in permeability, has already been sufficiently described in the preliminary report. It is reproduced here because it shows how sensitive the varnishes are to the artificial exposure used and how, in spite of the rapidity with which most of them went to pieces, they nevertheless graded themselves in accordance with practical experience. Table VI accompanies the figure, and the compositions are as follows:

CLASS.	COMPOSITION.	PER CENT.
<i>BA</i>	Rosin.....	45.0
	Benzine.....	55.0
<i>BC</i>	Dammar.....	43.5
	Turpentine.....	56.5
<i>BF</i>	Rosin.....	33.9
	Driers.....	2.4
	Chinese wood oil.....	20.5
	Linseed oil.....	3.4
	Benzine.....	40.0
<i>BE</i>	Rosin.....	40.4
	Linseed oil.....	24.0
	Benzine.....	35.4
	Driers.....	0.2
<i>BN</i>	Rosin.....	11.5
	Chinese wood oil.....	18.0
	Driers.....	1.4
	Linseed oil.....	18.6
	Benzine.....	50.5
<i>BK</i>	No. 1 kauri.....	29.5
	Linseed oil and driers.....	18.8
	Turpentine.....	27.7
	Benzine.....	24.0
<i>BH</i>	No. 3 kauri.....	26.0
	Linseed oil.....	21.0
	Driers.....	0.3
	Turpentine.....	18.7
	Benzine.....	38.7
	Rosin.....	0.3

CLASS.	COMPOSITION.	PER CENT.
BM.....	Kauri.....	14.1
	Linseed oil.....	27.4
	Turpentine.....	52.5
	Benzine.....	4.8
	Rosin.....	1.0
	Driers.....	0.2

BL, equal volumes of BK, bodied linseed oil, and turpentine;
 FL, Indian gum in turpentine; BG, equal volumes of BF and BN;
 BI, same formula as BN, except for an increase in rosin content.

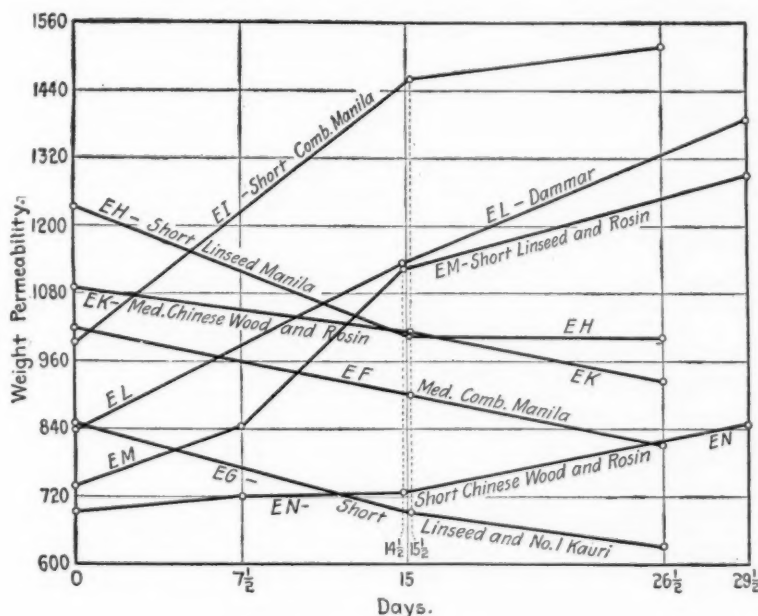


FIG. 17.—Interior Varnishes.

Interior Varnishes.—The kind of exposure to which the above varnishes were subjected was evidently too severe for the interior propositions. Consequently, eight of them (not all the same liquids) were attached to the under side of the lighting cover so that they would receive the light and heat of the day in the tank, but not the rain and cold of the night. Fig. 17, with Table VII, gives the result of such a process of testing, being an extension of the former report

on these panels. It is seen that although these interior coatings increased in permeability from the start under the exterior exposure (with rain and cold), many of them show the usual initial decrease in perviousness before the breaking-down point under the interior exposure (without the rain and cold). They grade in general, with the exception of *EI*, in accordance with experience with such mixtures of gums and oils. Doubtless a systematic investigation of both exterior and interior varnishes would reveal by this process some startling differ-

TABLE VII.—ADDITIONAL RESULTS ON INTERIOR VARNISHES (FIG. 17).

Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.	
				Actual	By Weight			Actual	By Weight			Actual	By Weight
<i>EL</i> ₁	3.09	1	0	3804	1308	2	15½	5630	1739	3	11	5804	1793
<i>EL</i> ₂	3.24	1	0	2085	677	2	15½	3661	1186	3	11	3831	1241
Average.....		1	0	2949	992	2	15½	4645	1462	3	11	4817	1517
<i>EL</i> ₃		3	15	3274	1390
<i>EM</i> ₁		4	15	2719	1290
<i>EN</i> ₁		4	15	1899	846

ences in liquids now selling at the same price. These coatings are identified as follows:

CLASS.	COMPOSITION.	PER CENT.
<i>EF</i>	Manila copal.....	22.7
	Linseed oil.....	17.6
	Driers.....	0.1
	Chinese wood oil.....	10.6
	Rosin.....	2.6
	Benzine.....	21.6
	Turpentine.....	24.6
<i>EH</i>	Manila copal.....	28.5
	Linseed oil.....	17.2
	Driers.....	0.3
	Turpentine.....	54.0

EG, the same as *BK*; *EI*, Manila copal with decreased amounts of Chinese wood and linseed oils, thinned as usual; *EK*, the same as *BI*; *EL*, the same as *BC*; *EM*, the same as *BE*; *EN*, the same as *BF*.

Barn Paints.—The results on the testing of barn paints are also reproduced, with one additional interval, the data for

which are furnished in Table VIII. The strong point about Fig. 18 is its exposure of the effect of the presence of an inferior resin, in an oil paint. Other curves were given in the pre-

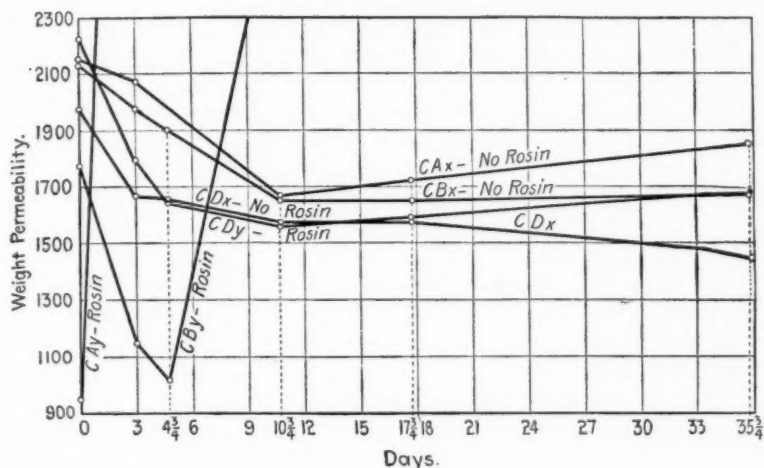


FIG. 18.—Barn Paints.

liminary report which emphasized the harmful effect of even traces of such adulterants—for they amount to as much—on the wearing qualities of an otherwise good mixture. However,

TABLE VIII.—ADDITIONAL DATA ON BARN PAINTS (FIG. 18).

Class.	Measurement No.	Interval, days.	Permeability.	
			Actual.	By Weight.
CDy ₁	6	18	2140	1670
CDy ₂	6	18	1920	1695
Average.....	6	18	2030	1682
CDx ₁	6	18	2125	1540
CDx ₂	6	18	2056	1390
Average.....	6	18	2090	1465
CBx ₁	6	18	3174	1687
CBx ₂	6	18	3103	1675
Average.....	6	18	3138	1681
CAx ₁	6	18	4186	2015
CAx ₂	6	18	3693	1700
Average.....	6	18	3939	1857

CDy, which contained only a trace of rosin, holds up remarkably well. CAy and CBy do not contain more than the usual amounts found in cheap barn paints and the illustration indi-

cates how truly money is wasted in the purchase of such materials. The formulas for the barn paints appear below:

CLASS.	COMPOSITION.	PER CENT.
<i>CDy</i>	Zinc oxide.....	18.9
	Silica.....	16.4
	Calcium carbonate.....	5.0
	Barium sulfate.....	11.6
	Linseed oil.....	21.8
	Water.....	11.1
	Japan.....	15.2
<i>CDx</i>	Zinc oxide.....	16.0
	Lead sulfate.....	4.0
	Silica.....	19.7
	Barium sulfate.....	10.0
	Linseed oil.....	20.0
	Water.....	7.8
	Japan.....	22.5
<i>CBy</i>	Iron oxide.....	4.9
	Calcium and magnesium carbonates.....	41.8
	Silica.....	2.5
	Linseed oil.....	15.4
	Water.....	5.0
	Japan.....	30.4
<i>CBx</i>	Iron oxide.....	3.5
	Calcium and magnesium carbonates.....	21.0
	Silica.....	10.1
	Linseed oil.....	23.8
	Water.....	19.1
	Japan.....	19.1
<i>CAy</i>	Iron oxide.....	3.9
	Calcium and magnesium carbonates.....	41.6
	Silica.....	0.6
	Calcium sulfate.....	1.3
	Linseed oil.....	9.3
	Water.....	3.1
	Japan.....	40.2
<i>CAs</i>	Iron oxide.....	2.3
	Calcium and magnesium carbonates.....	21.0
	Silica.....	10.1
	Linseed oil.....	25.6
	Water.....	20.5
	Japan.....	20.5

Six Red Leads.—Fig. 19 is an extension from the former report, with one coating omitted because no further measurements had been made on it. The additional results are given

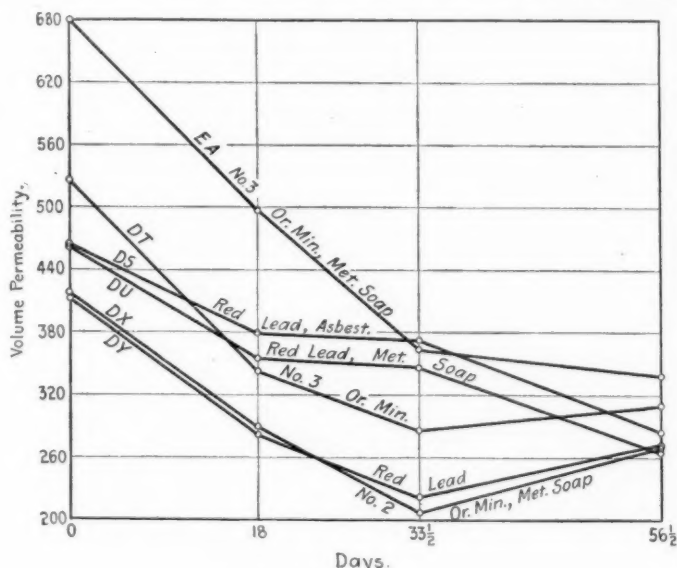


FIG. 19.—Six Red Leads.

in Table IX. The curves are reproduced, chiefly in order to illustrate the advantage of reporting on the basis of equal volumes of coat, as explained in Fig. 40. These films differed

TABLE IX.—ADDITIONAL RESULTS ON SIX RED LEADS (FIG. 19).

Class.	Measurement No.	Interval, days.	Permeability.	
			Actual.	By Volume.
DS ₁	4	23	248	218
DS ₂	4	23	472	350
Average.....	4	23	360	284
DT.....	4	23	502	309
DU.....	4	23	357	264
Dx.....	4	23	420	269
Dy.....	4	23	413	271
EA.....	4	23	785	338

in specific gravity, as may be inferred from the tabulation, all the way from 1.65 to 2.49, and if they had been reported on the weight basis, they would have appeared to differ in per-

meability more than they really do. It is noticed that three of them decrease and three increase in porosity during the last interval. Of those that decrease, two contain metallic soap and one asbestine; of those that increase, only one contains metallic soap, the other two being straight lead propositions. The chief surprise is in the No. 2 and No. 3 orange minerals and in the fact that the straight red lead has the same volume permeability as the former. Supposedly, the low permeability of the soap and the comparatively low density of the orange mineral counterbalance the high density and high permeability of the red lead. The addition of the soap to the red lead seems to have been more beneficial than to the orange mineral. The asbestine also had a favorable effect. However, too much importance is not to be attached to this set of tests, as only one duplicate was run.

The following data are self-explanatory:

CLASS.	COMPOSITION.	PER CENT.
<i>DS</i>	Red lead.....	39.0
	Magnesium silicate.....	31.0
	Linseed oil.....	30.0
<i>DX</i>	No. 2 orange mineral.....	53.4
	Metallic soap.....	5.1
	Linseed oil.....	41.5
<i>DT</i>	No. 3 orange mineral.....	51.8
	Linseed oil.....	48.2
<i>DY</i>	Red lead.....	75.0
	Linseed oil.....	25.0
<i>DU</i>	Red lead.....	53.4
	Metallic soap.....	5.1
	Linseed oil.....	41.5
<i>EA</i>	No. 3 orange mineral.....	53.4
	Metallic soap.....	5.1
	Linseed oil.....	41.5

Composite Whites.—Fig. 20, with Table X, is designed to show the effect of varying the composition slightly from that of a standard outside white. The standard is a lead-zinc-silica proposition, and small amounts of other constituents were added

thereto in order to ascertain whether the process under present conditions was sensitive enough to detect much difference. The answer should perhaps be in the negative, though the author does not doubt that the proper exposure would show up considerable differences. In *FT* no drier was used other than the active pigments. In *FD*, a much larger proportion

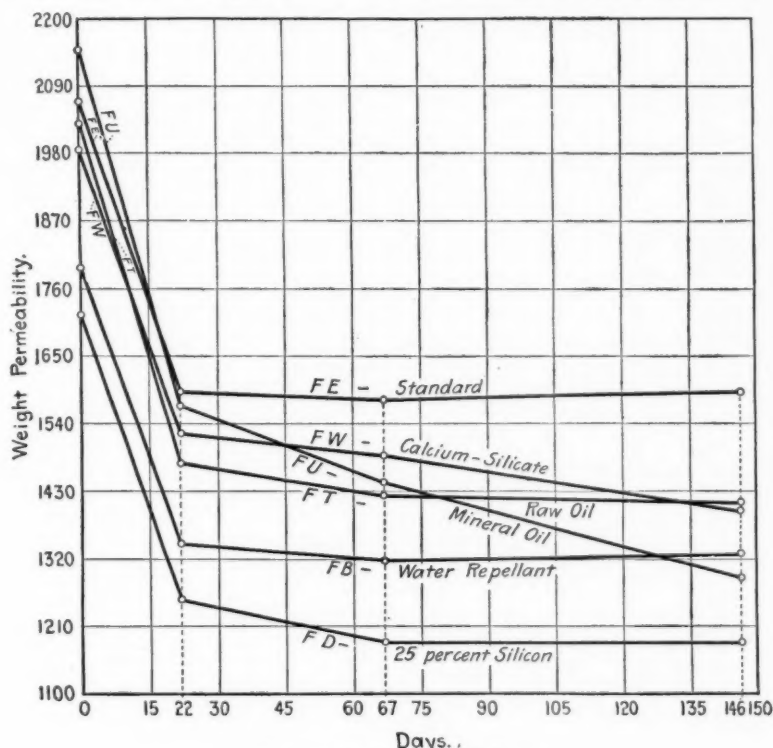


FIG. 20.—Composite Whites.

of silica was introduced. In *FU*, a trace of mineral oil was added and this seems to have had a beneficial effect.¹

Photographic View.—The fact has been mentioned that, after the artificial exposure, experts have graded sets of these special panels with the eye in the same order as they graded by their permeabilities. The higher their perviousness (the

¹ The compositions of these materials have not been furnished.

more water they let through), the worse they looked. Many photographs could have been presented, both ordinary and microscopic, in proof of this point. Fig. 21 is reproduced now simply as a concrete illustration, it being a view of the condition of barn paint CA_{y1} after three days' exposure. The trouble with this paint was its binder, and in general it may be said of the foregoing results that the binder is seen to play a more im-

TABLE X.—RESULTS ON COMPOSITE WHITES (FIG. 20).

Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.		Measurement No.	Interval, days.	Permeability.	
				Actual	By Weight			Actual	By Weight			Actual	By Weight
FB ₁	8.79	1	0	2045	1798	2	22	1537	1351	3	45	1561	1372
FB ₂	8.16	1	0	2192	1789	2	22	1645	1342	3	45	1551	1266
Average.....		1	0	2118	1793	2	22	1591	1346	3	45	1556	1319
FB ₁		4	79½	1608	1412
FB ₂		4	79½	1530	1249
Average.....		4	79½	1569	1330
FD ₁	8.42	1	0	2010	1692	2	22	1461	1230	3	45	1363	1148
FD ₂	8.80	1	0	1981	1743	2	22	1452	1278	3	45	1385	1220
Average.....		1	0	1995	1717	2	22	1457	1254	3	45	1374	1184
FD ₁		4	79½	1352	1138
FD ₂		4	79½	1402	1231
Average.....		4	79½	1377	1184
FE ₁	10.64	1	0	1969	2095	2	22	1517	1614	3	45	1471	1566
FE ₂	9.82	1	0	2074	2037	2	22	3	45	1626	1595
Average.....		1	0	2021	2066	2	22	1557	1592	3	45	1548	1580
FE ₁		4	79½	1482	1580
FE ₂		4	79½	1645	1612
Average.....		4	79½	1563	1596
FT.....	10.35	1	0	1960	2029	2	22	1419	1478	3	45	1375	1424
FT.....		4	79½	1369	1413
FU.....	9.89	1	0	2173	2149	2	22	1588	1571	3	45	1460	1444
FU.....		4	79½	1308	1290
FW ₁	9.66	1	0	2022	1953	2	22	1574	1520	3	45	1489	1447
FW ₂	9.71	1	0	2075	2015	2	22	1572	1526	3	45	1586	1540
Average.....		1	0	2048	1984	2	22	1573	1523	3	45	1537	1493
FW ₁		4	79½	1483	1430
FW ₂		4	79½	1408	1370
Average.....		4	79½	1445	1400

portant role than the pigment and therefore more exact information along this line is greatly to be desired.

Finally it can be stated in reference to the practical method of paint and varnish testing just outlined, that the results seem to be generally decisive, even for the outside whites, as shown in Fig. 13 of the preliminary report, and it may be concluded that such measurements during both natural and artificial exposure would be well worth the time and expense in the useful information that not only the manufacturer but also the public would thereby gather.

(D) THE CONSTANT-TEMPERATURE ROOM.

The Construction.—Rooms designed to maintain a uniform temperature within narrow variation limits are common in scientific laboratories over the world. But they are expensive and unsuited to a process of testing designed for a factory laboratory. Figs. 22 and 23 (Plate III) show horizontal sectional and vertical sectional views, respectively, of the testing



FIG. 21.—One of the Special Panels after Exposure of 3 Days.

rooms located at the University of Mississippi. The ceilings of the rooms are about 3 ft. below ground. The construction can be made inexpensive by locating the rooms at any convenient vacant space in the factory, as was done at the plant of the Patton Paint Co., already referred to. Fig. 5 is a partial view of this room.

Figs. 22 and 23 (Plate V) show the constant-temperature room, and the room for artificial exposure (2). The latter has a window (3) with southern exposure and a table (4) for the

balance. The brick walls of the building and the tile drain (6) for the earth below the floor of the cellar are also shown. The rooms are entered by the stairs (23) and the far corners are cut off by the partitions (7). The walls (8) of the rooms proper give a 12-in. air space all around. The walls of the inner room are not necessarily circular, but may be multi-angular, as shown in Fig. 5. This room is entered by the double doors (9). The flues (10, 11, 12, 13) enter from the outside air (or from the room below, if with a situation above ground) through the air space between the floor sills, jutting up to the floor. Flue No. 12 reaches preferably the exact center. The flues (14, 15, 16, 17) pass along the air space between the sills above the ceiling to the outside air (or to the room above), starting from the ceiling, flue No. 16 leading preferably from the exact center. There are thus two systems of ventilation for each room, and both rooms can be maintained at constant temperature. It is at least necessary that the inner one be so constructed, for it contains the testing devices. These testing devices rest upon the shelves (20), and gas enters by the pipe (18). The electric current for the switchboard and for the artificial exposure enters at the connections (19, 22). In Fig. 22, No. 21 is a pillar and not a partition.

Photographic View.—Fig. 24 is a photograph of this room, showing the two upper flues. One of these is to furnish air to the room while occupied by the experimenter, and the other (upper and lower flue) is connected to a gas stove, so that the air which it uses and its products of combustion do not mix with the air of the room proper. The walls of the room are thoroughly enameled and crude calcium chloride is kept around in crocks or flower pots in order to keep the humidity tolerably constant and low. The room is therefore well adapted for drying panels under uniform conditions, and several of these (double form) may be seen over the crocks. On the middle shelf the testing bases are visible. To the right on the bottom shelf are the trays holding the weighed dishes.

The Gas Stove.—Any kind of stove, with an air-entrance flue, as well the ordinary stove pipe, and containing within it a sufficiently large gas burner operated by a pilot flame, would

PLATE V.
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 VOL. XIV, PART II.
 MUCKENFUSS ON PERMEABILITY TEST FOR
 PAINTS AND VARNISHES.

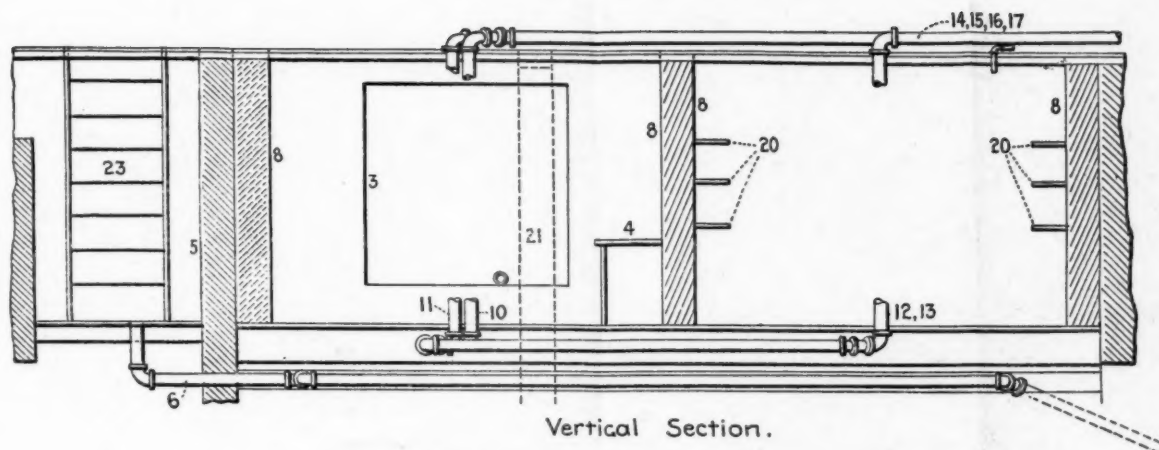


FIG. 22.—Vertical Section.

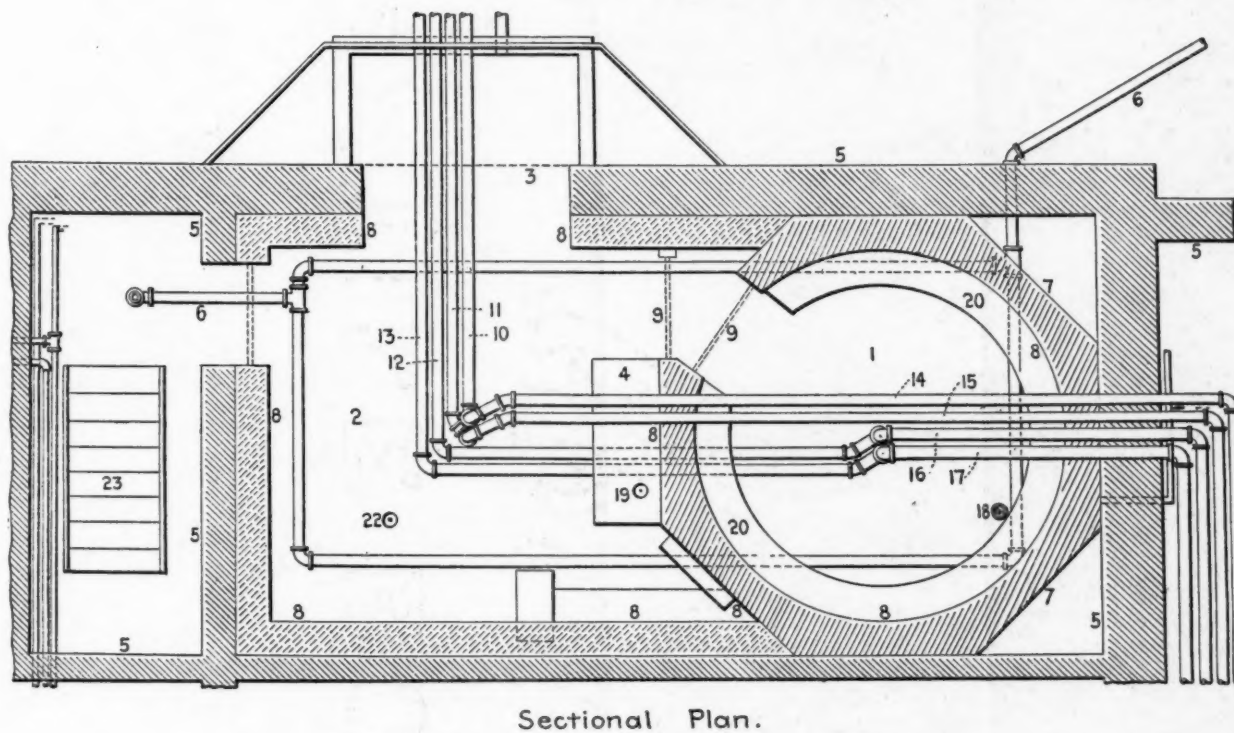


FIG. 23.—Sectional Plan.

be satisfactory; but the stove shown in Fig. 24, of which Fig. 25 shows vertical and horizontal sections, has the advantage of economizing heat, radiating it from the center of the room from floor to ceiling, and giving a wide range of temperature at which the thermometer may be held constant. Five radial gas burners (1), capable of being turned on singly or collectively,

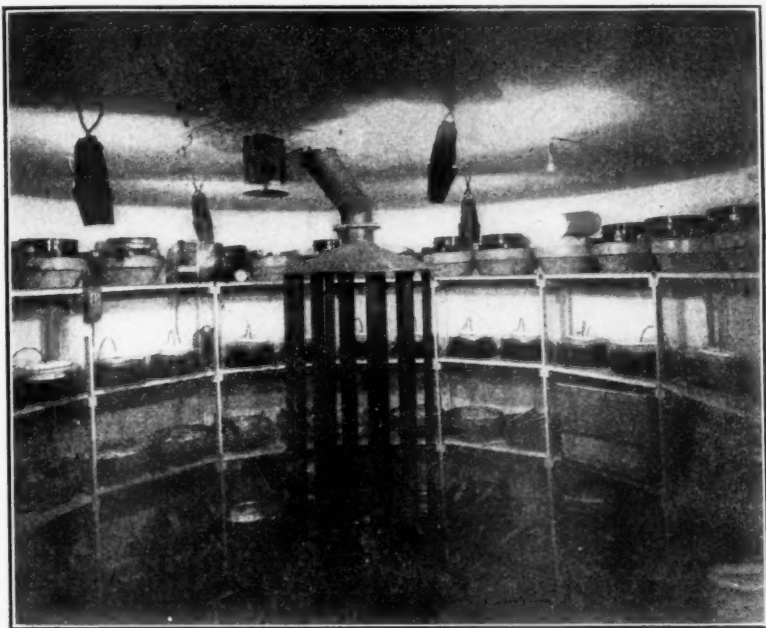
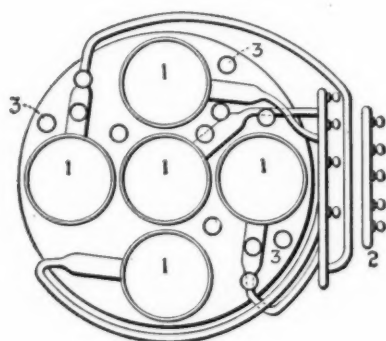
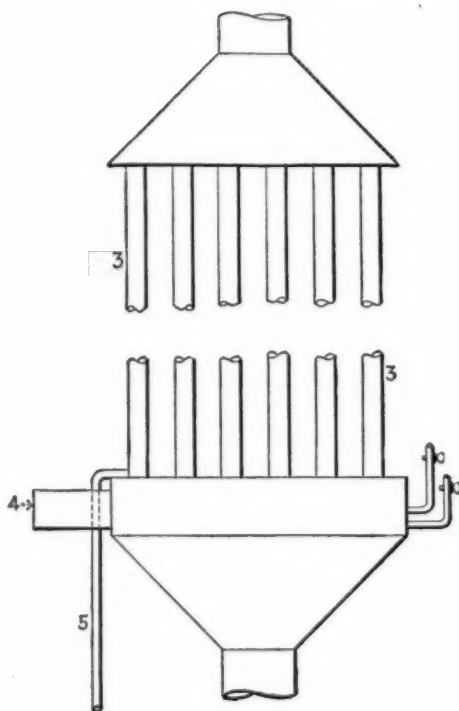


FIG. 24.—Dry Constant-Temperature Room at University of Mississippi.

and the pipe line (2) from which the pilot flames are distributed to these burners are shown, as are also the vertical pipes (3) twelve in all. Likewise, the peep hole (4), and one of the legs (5). One burner of this stove easily maintained the room at 80° F. in Mississippi and at 91°.2 F. in Milwaukee, so that it might well be constructed narrower, with only one burner. The difference in temperature maintained in the two rooms as above was due to the difference in location below and above ground.



Sectional Plan.



Elevation.

FIG. 25.—Gas Stove.

The Thermostat.—In Fig. 24, at the left and against the upper shelf, is seen the thermostat, made by the Minneapolis Heat Regulator Co., and adjusted to variations of $0^{\circ}.5$ F. Fig. 26 shows the instrument in detail, and Fig. 27 the motor which it operates by means of dry batteries. In Fig. 24, the thermostat appears with the contact end protected by a casing carrying a thermometer, the ring or sensitive end being open to radiation from the stove. Fig. 28 is a drawing to illustrate how the motor, which must be wound up by hand twice a day, operates the gas supply, which enters at *P*. The first branch pipe line goes to the pilot flames. At *B* the valve is worked by the lever *E*, balanced by the movable weight *W*. The arm *A* is moved through 180° , or half around, by the motor *M* at every closing of the circuit by the thermostat.

(E) MISCELLANEOUS PERMEABILITY RESULTS.

Literature of the Subject.—The literature of paint and varnish testing is voluminous and it would not therefore be possible to discuss it properly here, especially the recent work. In the field of practical testing, no better results have been obtained than those by the experts of this Society, as evidenced by its recent resumé on the subject.¹ The fact that preservative coatings for structural materials were more or less pervious to moisture has long been known, and this phase of the problem has been attacked in several ways. Simon,² by passing the electric current through coated iron plates, concluded that all paints were porous in greater or less degree. He believed from other experiments that a film may be impervious to the air and yet pervious to hot water. Using the paint film as the membrane in dialysis, he found the rate of diffusion of a crystalloid to increase

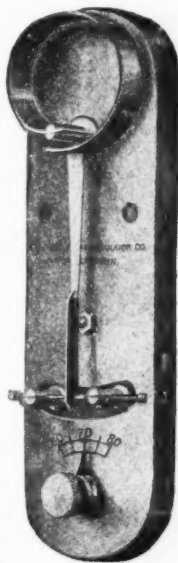


FIG. 26.—Thermostat.

¹ "Reports of the Committee on Preservative Coatings for Structural Materials," 1903-1913; published by the American Society for Testing Materials, Philadelphia, Pa. (1913).

² "Iron Corrosion, Anti-fouling, and Anti-Corrosive Paints," Andes-Salter, London, pp. 103-108 (1900).

rapidly after the first 24 hours. The permeability of paint and varnish films was measured by Treuman,¹ but the method used is not stated. Laurie's² work was largely qualitative, but interesting. He painted on glass over anhydrous copper sulfate and measured the permeability by the depth of blue color, also using the microscope. Amber was the only coating found impervious. Many investigators have coated glue, gelatine, wood, etc., and measured perviousness by the softening, swell-

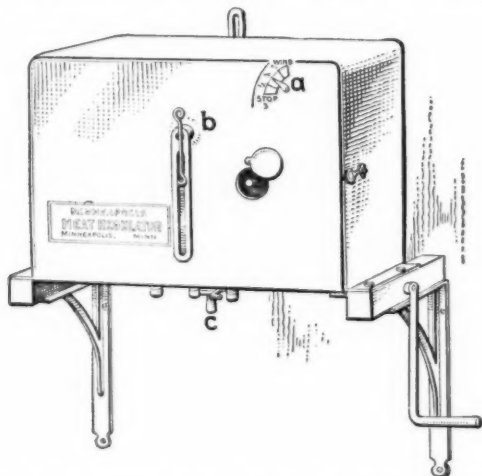


FIG. 27.—Motor Controlled by Thermostat, Operating Gas Supply.

ing, or increase in weight. Gardner and Perry's³ results were accurate, for they stretched films over mouths of bottles containing limewater and immersed them in carbon dioxide, the amount of precipitation of calcium carbonate being a measure of permeability to that gas. Also, the bottles containing sulfuric acid were kept in air saturated with moisture at constant temperature. More recently, others have attempted to rate the films quantitatively by means of their electric conductivities—Slade's⁴ work along this line being interesting. As regards artificial exposure, many authors have reported the use of steam, acids, and the like, as accelerators.

¹ *Journal, Soc. Chem. Ind.*, Vol. XVII, p. 1056 (1898).

² *Ibid.*, Vol. IX, p. 583 (1890).

³ *Bulletin No. IX*, Scientific Section, Paint Mfrs' Assn., p. 14 (1909); also *Bulletin No. XVIII*, p. 19 (1909).

⁴ *Journal of Industrial and Engineering Chemistry*, Vol. IV, p. 189 (1912).

Difficulty of the Problem.—There can be no question about the fundamental importance of the problem of reliable accelerated measurements of paint and varnish durability, nor about the admirable contributions to its solution made by the foregoing and other investigators. There are, as will be developed in this paper, so many conflicting factors involved that a thoroughly satisfactory solution can scarcely be hoped for. Unless such coefficients as the age of coating, temperature of measure-

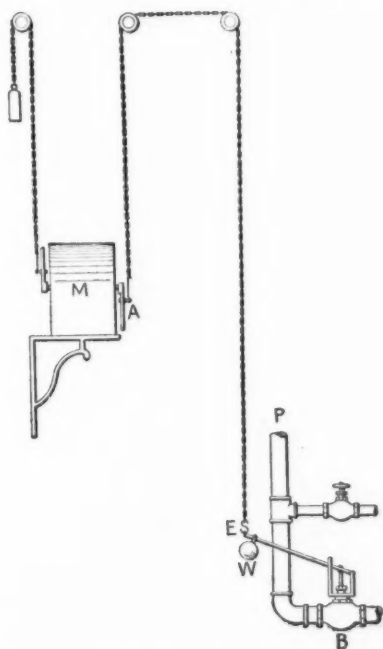


FIG. 28.—Operation of Gas Supply by Motor.

ment, and the intensity of sunlight, of heat, of humidity, and of mechanical effect during exposure, are taken into consideration, all results obtained are more or less qualitative and suggestive, rather than quantitative and conclusive. Even though all coatings compared be tested simultaneously under identical conditions, it is doubtful if the results could be reproduced, unless all factors entering into such results could be reproduced accurately.

Temperature and Permeability.—To prove the above contention and to indicate how sensitive permeability, as one of the properties of the protective coating, may be to the several conditions of measurement and exposure, let us consider the

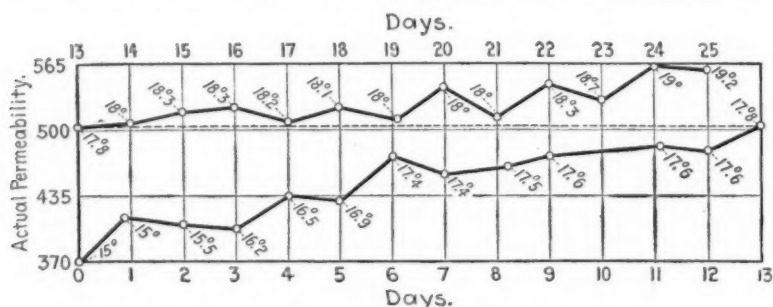


FIG. 29.—Effect of Temperature on Continuous Measurements.

effect of temperature alone. It was to be expected that measurements of permeability must be performed at constant temperature, and before this feature could be installed, the author made 25 successive measurements on a panel without ever removing

TABLE XI.—RESULTS ON EFFECT OF TEMPERATURE ON CONTINUOUS MEASUREMENTS (FIG. 29).

Measurement No.	Interval, days.	Actual Permeability.	Temperature, deg. Cent.	Measurement No.	Interval, days.	Actual Permeability.	Temperature, deg. Cent.	Measurement No.	Interval, days.	Actual Permeability.	Temperature, deg. Cent.
1	0.0	371	15.0	2	1.0	412	15.0	3	1.0	407	15.5
4	1.0	401	16.2	5	1.0	434	16.5	6	1.0	430	16.9
7	1.0	472	17.1	8	1.0	455	17.4	9	1.2	463	17.5
10	0.8	473	17.6	11	2.1	482	17.6	12	0.9	479	17.6
13	1.0	503	17.8	14	1.0	507	18.0	15	1.0	517	18.3
16	1.0	522	18.3	17	1.0	507	18.2	18	1.0	521	18.1
19	1.1	511	18.0	20	0.9	542	18.0	21	1.0	513	18.0
22	1.0	545	18.3	23	1.0	529	18.7	24	1.0	571	19.0
25	1.0	565	19.2

it from the testing device. The composition of the material was as follows:

Corroded lead	28 per cent.
Zinc white	12 "
Silica	10 "
Boiled linseed oil	50 "

The work was carried out in an air-tight box under the earth in a deep cellar. At the beginning and end of each measurement of permeability, the temperature in degrees Centigrade was recorded and averaged. The data are given in Table XI, and Fig. 29 gives the same diagrammatically. There were doubtless fluctuations of temperature in the box that were not recorded, especially as the effect is seen on the diagram generally to be retarded a day or two, but Fig. 29 plainly shows the gradual rise in permeability due to a gradual rise in temperature. Some allowance must also be made for inaccuracies of measurement at an early stage of the research.

Age of Coating.—As a further illustration of sensitiveness, four panels were coated with the same paint at intervals of 5 days and when the last was 5 days old, all four were measured.

TABLE XII.—VARIATION OF PERMEABILITY WITH AGE OF COATING.
(FIG. 30).

Order of Painting.	Weight of Coat, g.	Permeability.	
		Actual.	By Weight (7 g.).
1.....	7.65	1189	1214
2.....	7.54	1256	1236
3.....	8.23	1176	1277
4.....	8.26	1259	1366

The results are given in Table XII. The oldest gave as expected the smallest value. Fig. 30 illustrates this fact strikingly and indicates the truth that permeability and presumably all other properties of the paint and varnish film are continually changing from the very moment of application of the coat.

Condition of Absorbent.—Even the state of the absorbent has an appreciable effect, if it has been used a long time in making permeability measurements. Thus, on successive days, using alternately new and old calcium chloride, the results on the same panel were 1830 and 1735, and in another experiment 1110 and 1024, the higher figure being in each case obtained naturally from the newer and more efficient absorbent. This error is not great, if ordinary care is exercised.

Coats on Same or Opposite Sides.—Results vary also for two coats of the same weight of the same paint according as

applied on the same or opposite sides. Table XIII gives record on four panels prepared to test this point (unpainted side adjacent to the water while measuring, for panels Nos. 1 and 2, painted on only one side).

Panels Nos. 1 and 3 were measured two days later than Nos. 2 and 4; this and a possible difference in temperature will account for variations, which neutralize each other in the value for the average. Painting on both sides of the special

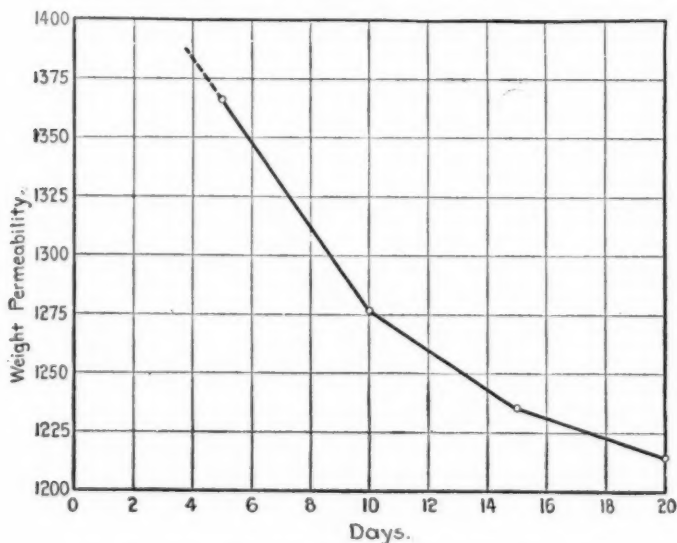


FIG. 30.—Variation of Permeability with Age of Coating

panels thus produces the lower permeability, at least when the imitation wood (paper) surfaces are used.

Yellow Coloration of Calcium Chloride.—Many measurements have been made of the permeability of paint and of oil films as soon as applied to the panels, always with high results compared to later values. It was observed in these cases that frequently the c.p. fused calcium chloride used as absorbent acquired a slight yellow coloration on the surfaces nearest the edges of the weighed dishes. To test this point, two rolls of matting were made, 8 in. high by 6 in. in diameter, with $\frac{1}{4}$ -in. strips of wood vertically every 45 deg. around on the roll, so

that successive layers would hardly touch each other. One roll was dipped in a commercial outside white containing corroded lead, zinc white, magnesium silicate, calcium carbonate, linseed oil, turpentine, and japan drier in ordinary proportions, and the other was dipped in boiled linseed oil. Both were placed on ground-glass plates, and upon each was placed a dish of the fresh calcium chloride. Each was then covered with an 8 by 10-in. bell jar in a dark closet.

At the end of 9 days, the absorbent of the first was colored yellow on the edges of the dish, while in the second case, or with the boiled oil, the absorbent began to turn yellow after 1 day and at the end of 5 days reached its maximum color, being then yellow throughout, though deepest in color on the edges

TABLE XIII.—VARIATION OF PERMEABILITY WITH METHOD OF APPLYING COATS.

Panel No.	Method of Applying Coats.	Weight of Paint, g.	Permeability.	
			Actual.	By Weight (7 g.).
1	Same side.....	7.75	1608	1880
2	Same side.....	7.15	1650	1685
Average				1682
3	Opposite sides.....	7.34	1325	1389
4	Opposite sides.....	7.13	1325	1361
Average				1375

of the dish. Some calcium chloride was placed in boiled oil for a week in a bottle and was not colored, showing, as was to be expected, that the color comes from a volatile product of the action of air on the paint or oil. Gardner¹ and other investigators by absorptive and spectroscopic methods have concluded that aldehydic or similar vapors emanate from the drying paint film. Whether these various effects are due to the same kind of emanations remains to be ascertained. This topic will be pursued further in the hope of identifying the yellow coloration observed.

Continuous Measurements.—It was explained in the preliminary report on this process of testing that paint was more

¹ *Journal of Industrial and Engineering Chemistry*, Vol. VI, p. 91 (1914).

permeable from the under side outwards than from the outer side inwards. Fig. 31 is reproduced now in testimony of this statement, with the addition of a zinc white and a lead-zinc coating. The results are given in Table XIV. In each of the five coatings used, the panels were retained on the bases continuously throughout the test, being given no exposure. Theoretically, the permeability should not have changed. The expression "upright" on Fig. 31 means that during the interval specified the panel was removed momentarily and replaced in a reverse position with the painted side up, away from the water. Conversely, "inverted" means that at this point the panel

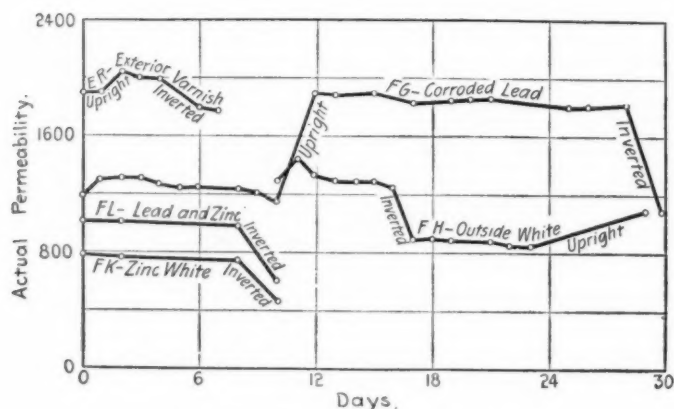


FIG. 31.—Continuous Measurements.

was lifted, inverted, and replaced with the painted side down, next to the water. In the former case, there is always a marked increase and in the latter a marked decrease in the permeability, but as long as the position of the panel is not changed, the results from day to day are seen to be practically constant. The results of an immediate measurement, or one as soon as the panel is placed on the testing base, are seen to fluctuate more than the others; hence the rule not to make a measurement until a panel has remained on a testing base 12 hours. The varnish showed very little difference between upright and inverted positions. It was a standard turpentine, long oil

(linseed), No. 1 kauri proposition. The composition of the paints was as follows:

CLASS.	COMPOSITION.	PER CENT.
FG.....	Corroded lead.....	59.6
	Boiled linseed oil.....	40.4
FK.....	Zinc white.....	34.8
	Boiled linseed oil.....	65.2
FL.....	White lead.....	36.9
	Zinc white.....	13.3
	Boiled linseed oil.....	49.8
FH.....	Corroded lead.....	15.0
	Zinc white.....	19.9
	Silica.....	3.5
	Lead sulfate.....	0.1
	Linseed oil.....	57.4
	Turpentine.....	2.9
	Benzine.....	1.2

TABLE XIV.—ADDITIONAL RESULTS ON CONTINUOUS MEASUREMENTS (FIG. 31).

ZINC WHITE.

Class.	Weight of Coat, g.	Measurement No.	Interval, days.	Actual Permeability.	Measurement No.	Interval, days.	Actual Permeability.	Measurement No.	Interval, days.	Actual Permeability.
FK ₁	13.32	1	0	802	2	2	783	3	6	764
FK ₂	12.86	1	0	776	2	2	771	3	6	727
Average.....		1	0	789	2	2	777	3	6	745
FK ₁		4	2	461
FK ₂		4	2	495
Average.....		4	2	478

CORRODED LEAD AND ZINC WHITE.

FL ₁		1	0	1026	2	2	1016	3	6	1007
FL ₂		1	0	1003	2	2	1014	3	6	958
Average.....		1	0	1014	2	2	1015	3	6	982
FL ₁		4	2	599
FL ₂		4	2	623
Average.....		4	2	611

Weathering Agencies.—Now that it has been graphically indicated by the foregoing results and by the testing of commercial materials how sensitive the permeability of the paint

and varnish coating is to every agency that may act upon it, it remains to consider these agencies separately. The drying and aging of such a film is a chemical reaction which has a definite velocity for each component of the coating. The influence of temperature, for example, upon the velocity of such

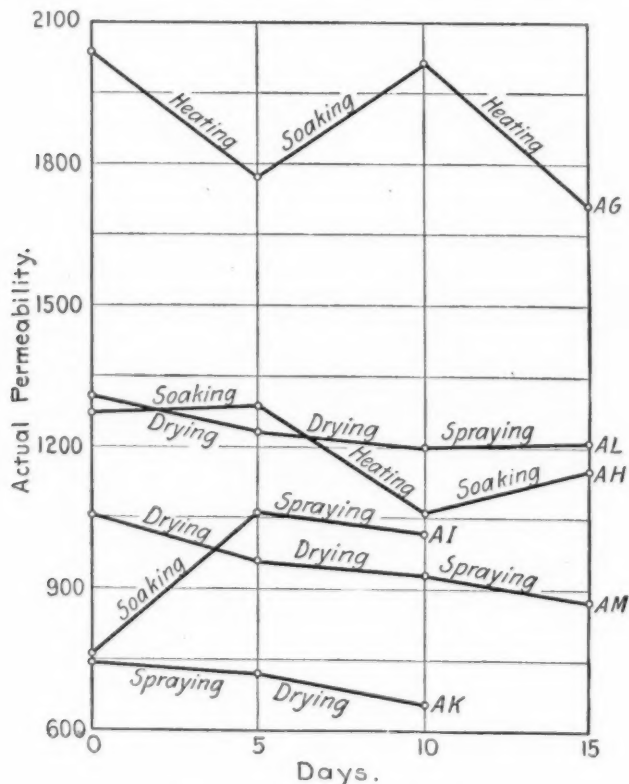


FIG. 32.—Weathering Agencies.

reactions is remarkable. Van't Hof showed that an increase of only 10°C . on an average doubles the velocity of a chemical change,¹ the increase being in geometrical ratio. This means that paint on the roof in summer at a temperature of 120°F . decays about 15 times as rapidly as in winter at 50°F .

¹ Bigelow, "Theoretical and Physical Chemistry," p. 364 (1912).

To illustrate this and similar points, six different commercial paints, mostly outside whites and grays, were subjected to various conditions, as shown in Fig. 32, and as the conditions could not be kept accurately constant, the contrasts between the coatings should not be magnified. The heating was in an oven at 70° C., the spraying with an ordinary garden spray, and the soaking in water at 30° C., all under conditions as identical as practicable, at the same 5-day interval, but not on the same dates. Two extra days in the dry room elapsed before a measurement was begun. Fig. 33 shows the method of spraying, except that it was not done on the roof. Drying

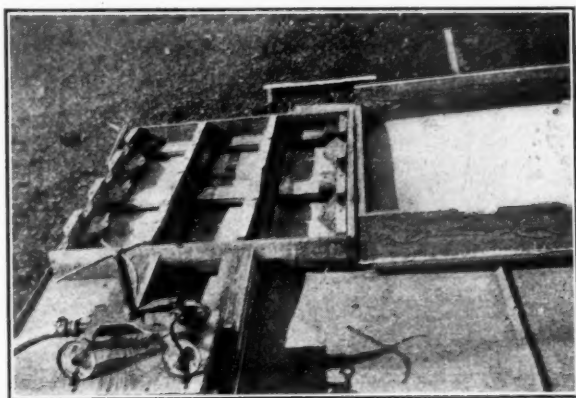


FIG. 33.—Method of Spraying Panels and of Exposure on Roof.

and heating both caused a decrease and soaking, an increase, as seen. In *AG* these changes are most marked and least in *AH*. Spraying has an intermediate effect between drying and soaking, probably because there occurs with it both the effect of water and of air. For example, *AM*, *AK*, and *AI* have great tendencies to dry and the permeability falls in spite of the water of the spray.

Effect of Light.—There is one important element in paint decay which has not been considered separately, namely, radiant energy as manifested in sunlight, apart from its heating effect. In order to test this point, eight panels were constructed on the basis of a constant weight of boiled oil, as follows: *AN*,

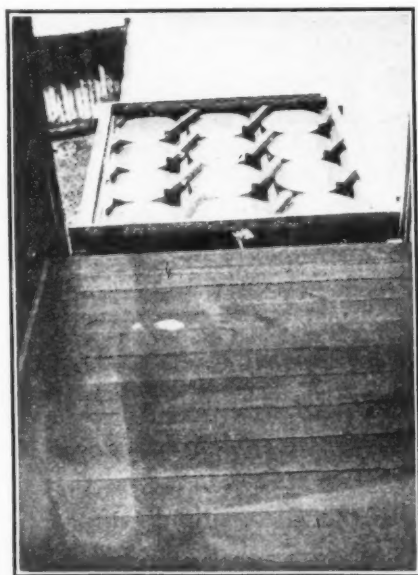


FIG. 34.—Exposure of Panels to Heat and Light without Rain.



FIG. 35.—Exposure of Panels to Heat without Light and Rain.

with the same composition as *FG* previously referred to; *AO* the same as *FK*; *AP* the same as *FL*, with half the proportion of lead in *AN* and half that of zinc in *AO*; *AQ*, made by adding 36.3 per cent of barytes to *AP*. Permeability and exposure tests were begun and ended on each set of duplicates on the same dates and on all sets at about the same time. After a first

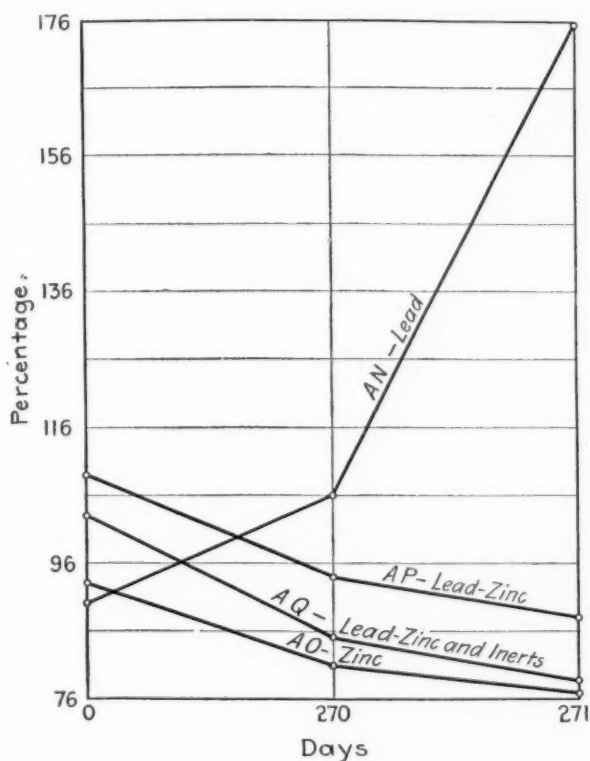


FIG. 36.—Effect of Light.

measurement they were exposed for about 270 days to the direct sun under such conditions that they were always removed indoors during a rain. Fig. 34 shows the actual arrangement for sliding them in and out of a window. One panel of each formula was covered with black paper. Fig. 35 shows the plan for keeping them under glass. After the long exposure, a second

measurement was made of permeability; then the films were placed in water for 24 hours, dried 1 day, and the third measurement made. Temperature was not controlled during the measurements; therefore the absolute values for permeability would be meaningless, but since the tests were made simultaneously, the relative values for the results are conclusive. In each case, therefore, the result on the film exposed to light and heat was multiplied by 100 and divided by that for the duplicate film exposed to heat alone. This is what is meant by "percentage" on Fig. 36 and in Table XV. Since the object of this experiment was not a comparison of the different

TABLE XV.—EFFECT OF LIGHT (FIG. 36).

Class.	Initial Weight Permeability.	Per cent. Light and Heat, of Heat.	Kind of Exposure.	Weight Permeability (after 270 days).	Per cent. Light and Heat, of Heat.	Weight Permeability after Soaking.	Per cent. Light and Heat, of Heat.
AN.....	951	..	Heat	2500	..	2140	..
	852	90	Heat and light	2664	106	3761	176
AO.....	541	..	Heat	1425	..	1645	..
	506	93	Heat and light	1154	81	1273	..
AP.....	628	..	Heat	1254	..	1343	77
	687	109	Heat and light	1178	94	1185	88
AQ.....	987	..	Heat	979	..	1000	..
	973	99	Heat and light	831	85	787	79

coatings, the equipment at hand at the time being insufficient for all purposes, check results were not obtained.

It seems hardly possible that the slightly higher temperature under the black paper would account for the differences indicated; for example, the notable gain in permeability of the lead panel exposed to light over its duplicate exposed only to heat, before and especially after contact with water. Hence the conclusion is in agreement with experience that radiant energy (sunlight) is an important factor in the aging of protective coverings. As to the comparison of the different curves of Fig. 36, it will be noticed that the results are not in entire agreement with those given for the artificial exposure of Fig. 10, but it must be remembered that these are the only data presented in this paper for ordinary exposure without rain, that

the length of time, 270 days, was much longer than that used artificially, and that the light came from the south side of a house in the summer in a southern state (Mississippi). It is proposed by the author next to make a complete series of measurements, using only natural exposure. Experiments have already been carried out on films exposed to heat and light during the day and to spraying at night¹ for 90 days, with marked increases in permeability.

Old Model of the Testing Device.—Fig. 37 shows the arrangement and Fig. 38 the photograph of the testing apparatus as originally used at the University of Mississippi. The jar (34) was filled with soda lime and the bottle (20) with water. The weighed U-tube (26) contained calcium chloride and weighed U-tube (25), soda lime, with a protecting tube of the lime, of

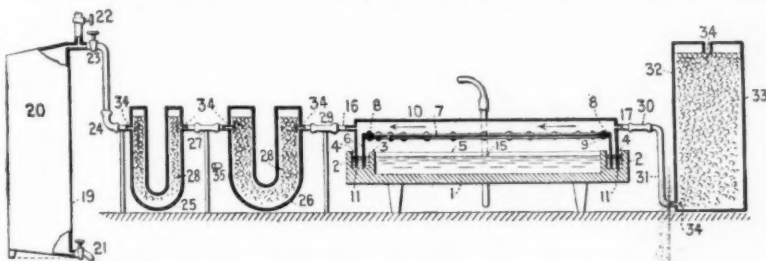


FIG. 37.—Arrangement of Old Model of Testing Device.

course, between it and the bottle. Over the water of the testing base and under the paint, carbon dioxide was passed slowly. To make a measurement, the water in the bottle was allowed to drop out slowly, after which the two tubes were again weighed. In this way, perviousness to carbon dioxide as well as to water was obtained. Gardner and Perry's² results were duplicated, though carbon dioxide permeability was always much the lower. Work of this kind was commenced in 1907 by the author at the plant of the Lowe Brothers Co., Dayton, O. He desires here to express to the officials of this company appreciation for the active encouragement of this research in its incipency.

¹ See Fig. 33 for illustration of the exposure frame.

² *Bulletin No. IX*, Scientific Section, Paint Mfrs. Assn., p. 14 (1909); also *Bulletin No. XVIII*, p. 19 (1909).

Additions to Linseed Oil.—Some work was done with the old model of the apparatus on various oils, except that weighed dishes were used by the present method and only initial permeabilities of the fresh films determined. It was found that each oil has its own constant and that adulterations of one oil with another could be detected to some extent. It is argued

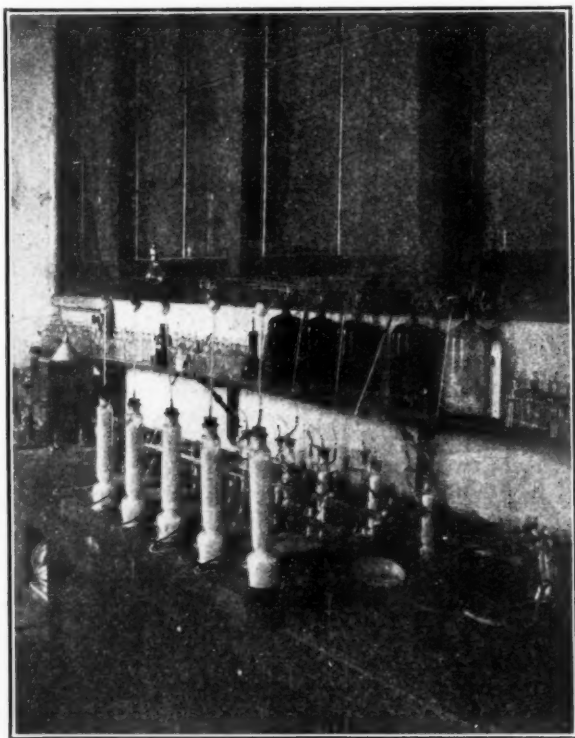


FIG. 38.—Old Model of Testing Device.

that in this way the permeabilities of substances, if worked out on a large scale, like specific gravity and index of refraction, could be used as an aid in the complex problems of proximate organic analysis. For example, Fig. 39 (a) with Table XVI shows that menhaden oil regularly increases the perviousness of the standard oil. In like manner, Fig. 39 (b) and Table XVI indicate the effect of small amounts of two other oils.

Oil Permeability.—What is meant by the statement that a paint has a high or a low permeability? The thicker the coating happens to be, the lower the actual permeability. For this reason, as explained in the beginning, results in this paper are generally given on the basis of weight of film (equivalent to film weighing 10 g.), but some coats go on thick and others spread thin; some liquids, as varnishes, are very light in weight and others, as red lead mixtures, are very heavy. How shall they be compared? A ready means of contrast of straight

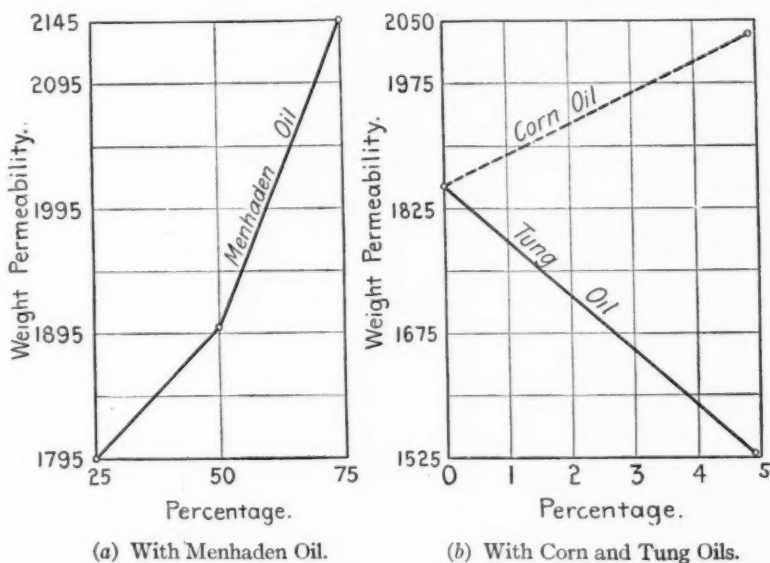


FIG. 39.—Adulteration of Linseed Oils.

oil propositions is on the basis of equal weights of fixed oil in the film. Since the average percentage of drying oil in protective coverings is not far from 40, the weight of oil corresponding to the standard 10 g. of coat would be 4 g. We might define "oil permeability," therefore, as the result obtained from a film of standard area spread with sufficient paint to contain 4 g. of fixed oil. The following proportion would hold good:

$$4 : \frac{\text{Wt. of film} \times \text{per cent of oil}}{100} = \text{actual permeability} : \text{oil permeability}.$$

Combining this with the equation for obtaining weight permeability, it is evident that multiplying results on the weight basis by the percentage of oil, divided by 100, and dividing the product by 4, will give the data on the oil basis.

The oil is the life of the covering and the dictum is still true that no paint is better than its binder. This is because the pigment or gum is added to protect the oil in much the same way as rock is used to ballast a roadway. Water is continually entering and emerging from the coating in actual service, the direction varying according as the humidity of the air is greater or less than equilibrium conditions within the

TABLE XVI.—RESULTS ON ADULTERATION OF LINSEED OIL (FIG. 39).

75 PER CENT MENHADEN OIL.			PURE LINSEED OIL.		
Measurement No.	Interval, days.	Actual Permeability.	Measurement No.	Interval, days.	Actual Permeability.
1	0	2145	1	0	1850
50 PER CENT MENHADEN OIL.			5 PER CENT TUNG OIL.		
1	0	1915	1	0	1530
1	0	1880			
Average	..	1900			
25 PER CENT MENHADEN OIL.			5 PER CENT CORN OIL.		
1	0	1835	1	0	2030
1	0	1790			
1	0	1765			
Average	..	1795			

coating at the time. The pigment or gum offers resistance to this moving water. Data on the oil basis will thus afford a comparison of the degree of resistance offered for equal quantities of oil.

Volume Permeability.—But many paint and varnish materials contain no fixed oil. A comparison, therefore, based on volume would seem to be the best basis. This is easily accomplished by dividing the weight permeability by the specific gravity of the material with volatile constituents removed, giving the "volume permeability," or the value for a volume of 10 cc. spread over the standard area.

Illustrations.—Fig. 40 gives the results comparing the volume basis with two other methods of reporting for a set of paints of great divergence in specific gravities. Vertical lines on the chart are apportioned among the several coatings, each designated by a letter; and any position on one of these lines shows the value obtained for the same duplicate panels on various bases of calculation. By the connection of points on the same basis, there is produced a graphic indication of the comparative degree of divergence for different methods of presenting results. It is evident that these liquids, though differing widely on the basis of equal number of coats or equal weight

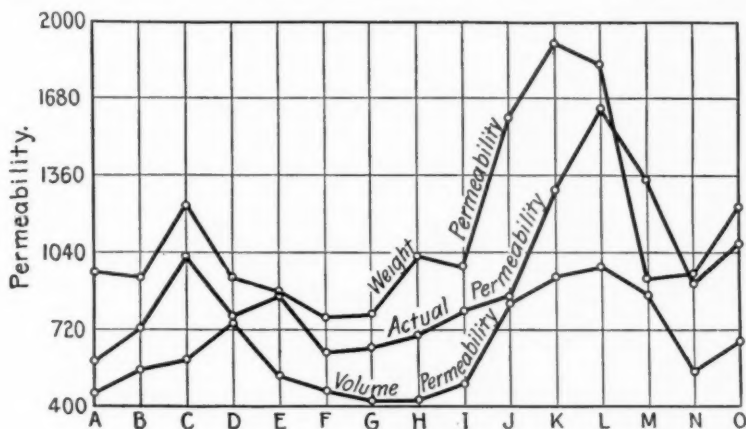


FIG. 40.—Volume Permeability.

of film, are not so far apart from the standpoint of equal thickness of coat. The latter curve is most nearly a straight line. This would indicate that the thickness rather than the density of a film is the determining factor in its perviousness.

Fig. 41 is given to compare the oil basis with actual and weight permeability, though the curves are constructed quite differently from Fig. 40. Here three vertical lines are set aside for the three bases of calculation and the lines connecting any three points identified by letter are for one particular material. The object is to bring out the individuality of the coatings. For example, the pigment in *Q* appears wasted, since it assists the oil very little in offering resistance to water, though pound

for pound this paint is seen to be an average in imperviousness. The opposite is true of *W*. In the cases of *C*, *H*, and especially of *K* and *L* of Fig. 40, the permeability is an average one, gallon for gallon, but pound for pound and coat for coat, the protection afforded is slight. This is not true of *D*, which is good by the pound, gallon, or coat (weight, volume, or actual

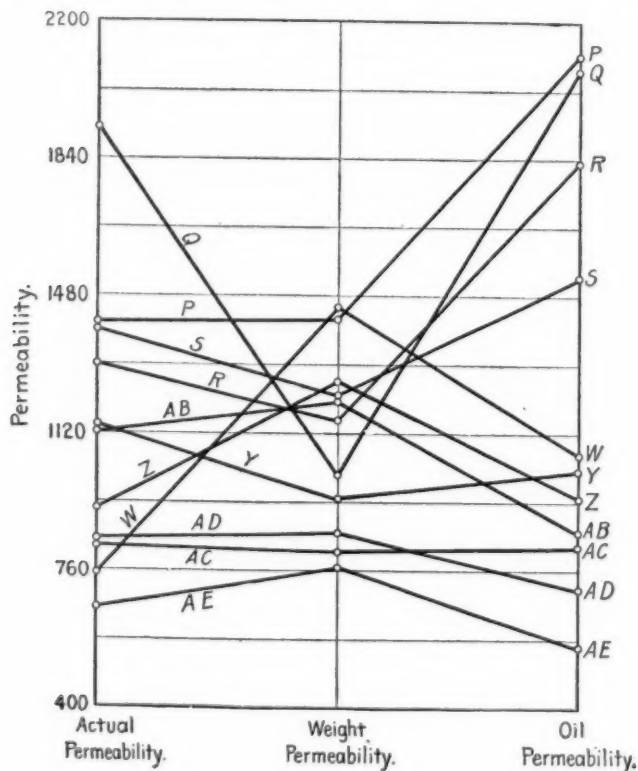
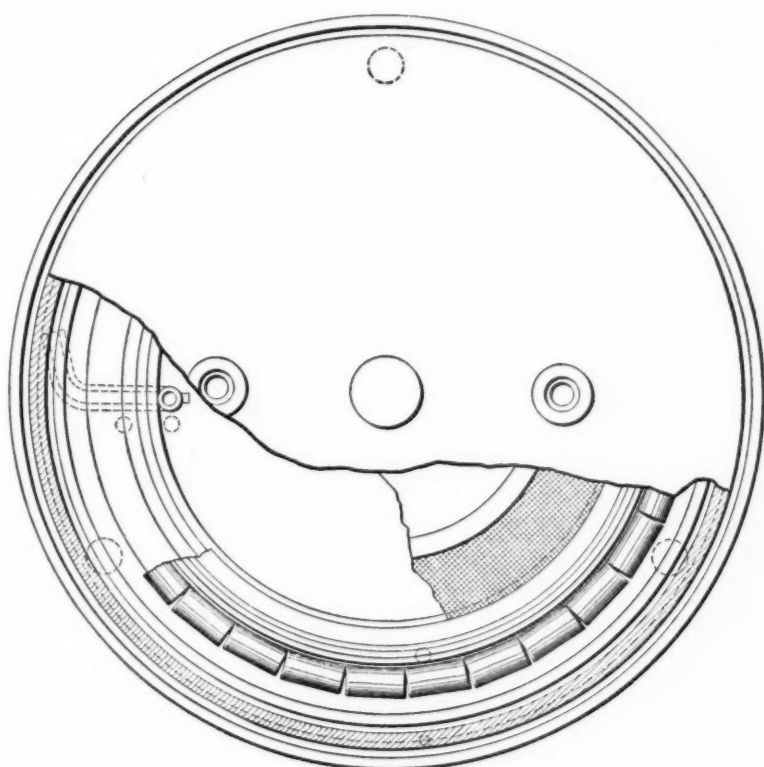


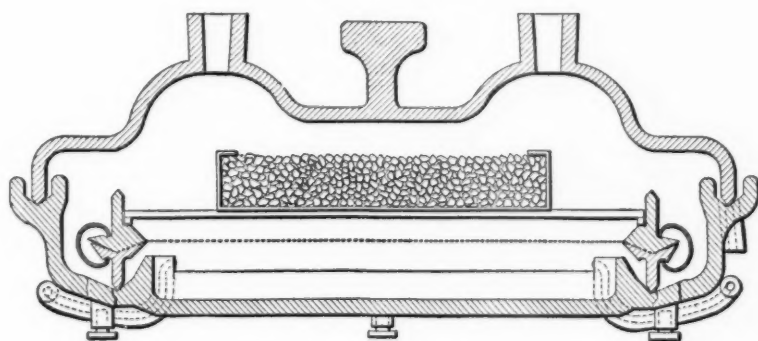
FIG. 41.—Oil Permeability.

permeability). Since the object is merely to clarify a definition of terms, there is no need to describe these materials further or to give the exact numerical results.

Ideal Form of Apparatus.—The materials used for the present testing device, though inexpensive and in some respects satisfactory, are not the best obtainable for the purpose. To



Sectional Plan



Vertical Section.

FIG. 42.—Ideal Form of Testing Device.

secure more accurate results, not only should the constant-temperature room be regulated more uniformly, even at different levels in the room, but the measuring apparatus should be of durable, tough materials, such as the newer synthetic resins, and of more suitable shape. Fig. 42 shows a vertical sectional view and a horizontal section of what may be termed the ideal form of the testing device, which needs no

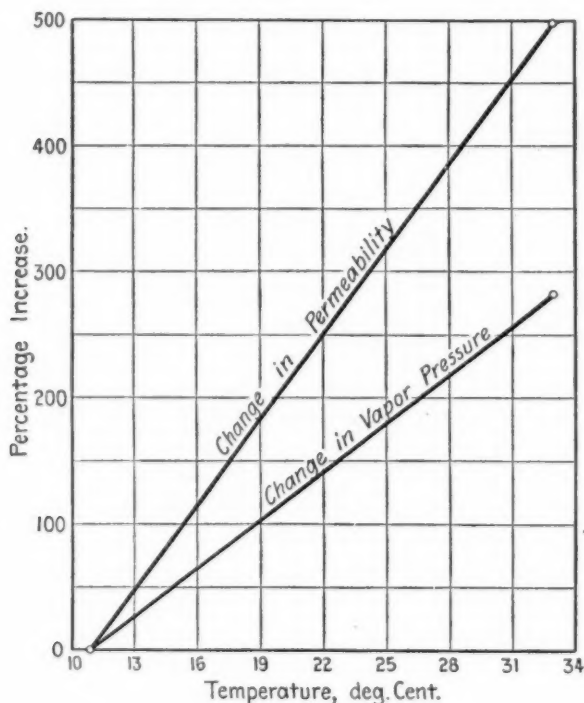


FIG. 43.—Permeability and Vapor Pressure.

further explanation at this stage of the research. Nor need any further statements be made in reference to the equipment for artificial exposure. When the kind and degree of concentration of weathering agencies that are fairly adapted to wearing out each kind of paint and varnish most rapidly have been ascertained by research, then an automatic equipment by which the panels would pass regularly through the successive kinds of

weather and could be inserted or removed at desired intervals would be ideal.

Nature of Permeability.—L. Dufour¹ years ago tested the rate of diffusion from moist to dry air, using porous diaphragms. He found that the rate of diffusion was proportional to the pressure of water vapor on both sides of the diaphragm and was apparently independent of temperature, since the vapor pressure itself is a function of the temperature. This diffusion took place even through marble 5 mm. thick and slowly through alabaster. In the case of porous earthenware, the rate of diffusion was inversely proportional to the square root of the thickness of the diaphragm, rather than simply inversely to the thickness. These facts, along with those presented in this

TABLE XVII.—EFFECT OF TEMPERATURE ON PERMEABILITY AND WATER-VAPOR PRESSURE (FIG. 43).

Measurement No.	Temperature, deg. Cent.	Actual Permeability.	Water-Vapor Pressure, mm.
1.....	32.9	853	37.16
2.....	10.94	137	9.75
3.....	10.84	139	9.69
4.....	32.9	802	37.16
Average 1 and 4.....	32.9	827	37.16
Average 2 and 3.....	10.89	138	9.72
Per cent increase in permeability.....	499.30
Per cent increase in water-vapor pressure.....	282.30

paper and those by the host of workers on osmosis following Graham's classic research, lead the author to the belief that permeability is an inherent property of matter² and that all substances will be found permeable, if not at one temperature, then at another; if not to one fluid, then to another. Many isolated facts point to this conclusion. He proposes with his students to make a systematic study of materials from this view point.

Permeability and Vapor Pressure.—While it may be true that diffusion through diaphragms having pores visible to the

¹ *Comptes Rendus*, Vol. LXXVIII, pp. 961-964 (1874).

² Muckenfuss, "Permeability Measurements as an Aid to Proximate Organic Analysis," *Science*, Vol. XXXIX, p. 363 (1914).

microscope is proportional to change in vapor pressure,¹ a parallelism of this kind is probably not true for non-porous membranes, such as the fresh paint, varnish, or oil film. Fig. 43 and Table XVII are an illustration in point. Permeability increases so much more rapidly than tension of aqueous vapor that some additional explanation must be offered for the discrepancy.

Absorption by Linoxin.—The absorption of water by linoxin offers a partial explanation. This material is most readily obtained from the sides of a linseed-oil tank that has been long in service. Four excellent samples were cleaned, dried, and

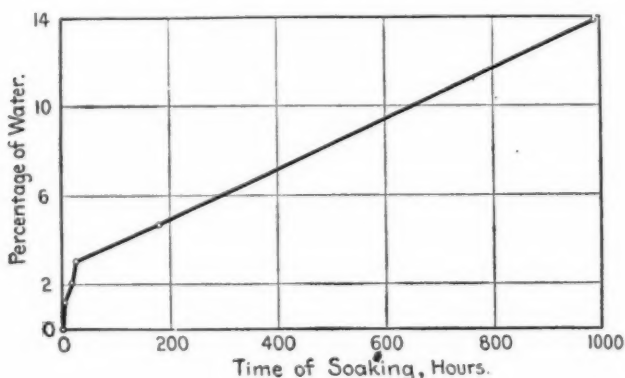


FIG. 44.—Absorption by Linoxin.

weighed in four weighing bottles. The results of testing these are given in Fig. 44 and Table XVIII. They were placed in four beakers of water for the number of hours interval stated at the temperature given, then removed, pressed dry with a good absorptive towel, replaced in the weighing dishes, and weighed, the increase in weight being recorded. This routine was repeated, as shown, until the samples of linoxin became of the consistency of soft gum and the pressing dry could not be continued.

¹ See the new International Encyclopedia, Vol. IX, p. 718 (1903), where the following formula is stated to hold good for fluids passing through capillary tubes:

$$V = \frac{Pr^2}{6nl}$$

where V = velocity of passage or rate of diffusion; P = difference in pressure; r = radius of tube; l = length of tube; and n = a constant.

Cause of Permeability.—The foregoing paragraph therefore suggests that the solubility of water in linolin is a determining factor in its passage in and out of a protective coating, which has dried linseed oil as its binder. This explanation for perviousness was defended by Kahlenberg¹ in this country and abroad by L'Hermit,² Flusin,³ Raoult,⁴ and others. For cases where the contact water is in the liquid state, we find several other explanations of permeability, namely, the kinetic theory defended by Van't Hof, the surface-tension theory, the attrac-

TABLE XVIII.—ABSORPTION OF WATER BY LINOLIN (FIG. 44).

Sample No.	Weight Dry, g.	First Record.				Second Record.				Third Record.			
		Time Soaking, hours.	Temperature of Water, deg. Cent.	Weight of Sample, g.	Total Water Absorbed, per cent.	Time Soaking, hours.	Temperature of Water, deg. Cent.	Weight of Sample, g.	Total Water Absorbed, per cent.	Time Soaking, hours.	Temperature of Water, deg. Cent.	Weight of Sample, g.	Total Water Absorbed, per cent.
1.....	54.69	2	40	55.26	1.0	16.5	30	55.80	2.0	8	36	56.20	2.7
2.....	49.86	2	46	50.47	1.2	16.5	31	50.94	2.1	8	39	51.34	3.0
3.....	49.51	2	55	49.95	0.5	16.5	32	50.35	1.7	8	47	50.95	2.9
4.....	40.97	2	67	41.96	2.4	16.5	34	42.14	2.8	8	55	42.71	4.2
Average					1.3				2.2				3.2

Sample No.	Weight Dry, g.	Fourth Record.				Fifth Record.				Sixth Record.			
		Time Soaking, hours.	Temperature of Water, deg. Cent.	Weight of Sample, g.	Total Water Absorbed, per cent.	Time Soaking, hours.	Temperature of Water, deg. Cent.	Weight of Sample, g.	Total Water Absorbed, per cent.	Time Soaking, hours.	Temperature of Water, deg. Cent.	Weight of Sample, g.	Total Water Absorbed, per cent.
1.....	54.69	145	24	57.37	4.9	125	24	58.42	6.8	642	24	62.48	14.2
2.....	49.86	145	24	52.29	4.9	125	24	53.10	6.5	642	24	57.26	14.8
3.....	49.51	145	24	51.70	4.4	125	24	52.46	5.9	642	24	56.42	14.0
4.....	40.97	145	24	43.18	5.3	125	24	43.80	6.9	642	24	46.94	14.5
Average					4.9				6.5				14.4

tion theory,⁵ and the cohesion theory.⁶ As far as the film itself is concerned, it does not appear to make much difference whether the contact water is liquid or vaporous, but some of the above theories would hardly hold good for vaporous contact. We are

¹ *Journal of Physical Chemistry*, Vol. X, p. 141 (1906).

² *Comptes Rendus*, Vol. XXXIX, p. 1177 (1854).

³ *Ibid.*, Vol. CXXVI, p. 1497 (1898); Vol. CXXXI, p. 1308 (1900).

⁴ *Ibid.*, Vol. XXI, p. 187 (1845); *Zeitschrift für Physikalische Chemie*, Vol. XVII, p. 737 (1896).

⁵ Bigelow, "Theoretical and Physical Chemistry," p. 217 (1912).

⁶ Metcalf, "Cause of Osmotic Pressure," *Science*, Vol. XXXIX, p. 362 (1914).

reduced practically to the kinetic and the solubility theories. If the former were entirely true, moisture permeability should vary with differences of vapor pressure, which, as just explained, Dufour and others found to be true for porous materials. There is no doubt about an old paint coating being full of visible pores, and Bigelow¹ and Bartell² have hypothecated these even for fresh diaphragms, and on the basis of the kinetic theory have calculated the pore diameters from the rate of passage of water through membranes simply under pressure. In considering this vexed problem of the cause of permeability and in spite of the above work, the author believes that we must distinguish permeability clearly from porosity. A sharp distinction should also be made between such expressions as osmosis and diffusion, though the latter term as used seems to have no definable significance.

The full meaning of the data presented in these pages must await the verdict of time. A certain state of impermeability is only one of the many qualities that a good protective coating should possess. How far changes in the former indicate changes in the latter may indeed be theoretically confirmed, but must in the end be decided from the practical and experimental view point.

(F) RESULTS.

1. A testing device has been explained by which the permeability of paint, varnish, and other coatings can be accurately determined at any stage of exposure.

2. The apparatus and method for exposure of such coatings on special panels to artificial weather conditions in varied successions are illustrated.

3. The design for a simple room, in which the permeability measurements and drying tests may be performed at a constant temperature, is given.

4. Commercial materials were tested by artificial exposure, with results as follows:

(a) Implement paints showed sharp contrasts, some breaking down in 3 days;

¹ *Journal, Am. Chem. Soc.*, Vol. XXIX, p. 1675 (1907).

² *Journal of Physical Chemistry*, Vol. XVI, p. 318 (1912).

(b) One outside white broke down in 81 days and standard pigments showed a divergence in 107 days;

(c) Much difference was observed between four drying oils in 107 days, as also between lead and manganese as driers;

(d) Metallic coverings contrasted greatly among each other all along during 114 days of exposure;

(e) Of two exterior enamels, one went to pieces in 13 days;

(f) Three cement coatings failed to hold up 2 days;

(g) Sharp contrasts were obtained among both exterior and interior varnishes with both exterior and interior artificial exposure. These results, as most of the others, seemed to agree with practical experience;

(h) The effect of small amounts of rosin in both outside whites and barn paints was early manifest in permeability changes during exposure.

5. Permeability was shown to be a property exceedingly sensitive to temperature changes and to many other conditions of measurement; also changing continually with the age of coating.

6. It was indicated that a paint covering is more pervious to water leaving, from the protected surface, than to the same entering, from the outside.

7. The effect of various weathering forces on permeability is shown.

8. Fused calcium chloride possesses the property of acquiring a yellow color through absorption of some emanation from the drying oil film.

9. By results on the old model of the testing device, several drying oils are shown to have definite permeabilities, pure or mixed.

10. The word, permeability, is defined with reference to quantity of moisture passing through, compared with the weight, volume, or percentage of oil in the coating.

11. The ideal form of the testing device and the ideal method for artificial exposure are considered.

12. The nature of moisture permeability is discussed in the light of change in vapor pressure and the absorptive power of the dried oil film.

DISCUSSION.

Mr. Kohr. **MR. D. A. KOHR.**—I shall show a few lantern slides that illustrate some of the results we have obtained in the paint research laboratory with which I am connected; which results seem to me have a bearing on Mr. Muckenfuss' excellent paper.

We believe the best way to test the durability of paint is by actual exposure tests; and we do this on testing racks made especially for the purpose.



FIG. 1.—Testing Racks in Winter.

The first slide (Fig. 1) shows these racks in the winter time; you will notice that the snow has partly melted.

The second slide (Fig. 2) shows these racks in the summer time with the panels exposed to the sunlight. Like Mr. Muckenfuss, we find the summer exposure to be the most severe. You will note that the panels are inclined at an angle of about 30 deg. They face southwest. While they are not large, the exposures are always made in duplicate, usually in triplicate or quadruplicate.

The third slide (Fig. 3) illustrates the result of a rather **Mr. Kohr.** rough-and-ready method of determining the initial permeability of a paint film by painting poplar boards with the paint, and subjecting them to immersion in water for a definite time. The top curve shows the blank, an unpainted panel. The next marked curve shows a carbon and linseed-oil paint; and the lower marked curve shows a zinc-oxide and linseed-oil paint. On the left is the weight of water absorbed in grams; at the bottom, the time in days. From this we infer that zinc-oxide



FIG. 2.—Testing Racks in Summer.

and linseed-oil paint is more impervious to moisture than carbon and linseed-oil paint.

The next slide (Fig. 4) shows a carbon and linseed-oil paint on steel after an exposure of $3\frac{1}{2}$ years. It illustrates the durability of a coating not very waterproof, as shown by the test just described.

In the next slide (Fig. 5) we see the condition, after 15 months' exposure on wood over a white lead primer, of zinc-oxide paint, which tested up as rather impervious in the water test. This illustrates the early failure of paint, which, by our

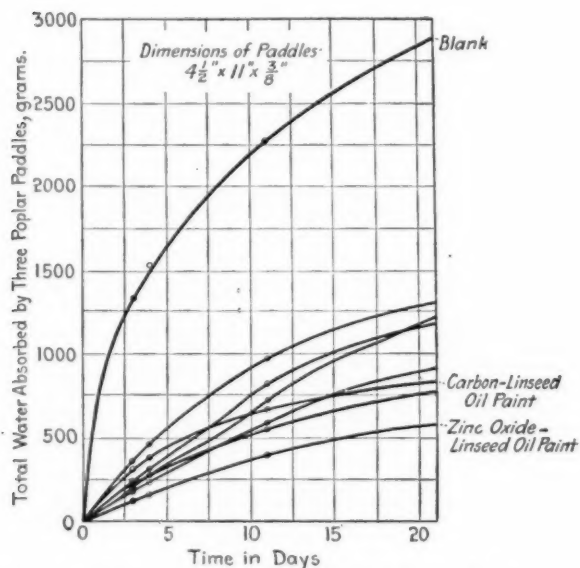


FIG. 3.—Method of Determining Initial Permeability of a Paint Film.

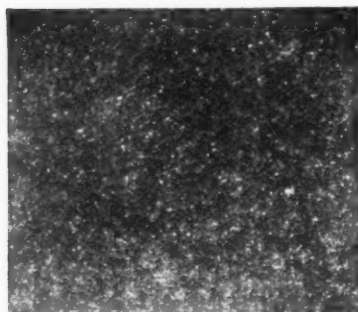


FIG. 4.—Carbon and Linseed-Oil Paint Exposed on Steel $3\frac{1}{2}$ Years.

test, was less permeable than the one shown in the last slide. Mr. Kohr. Both panels were three-coat work.

Not having at hand another slide, this one (Fig. 6) is offered as illustrating the durability of carbon and linseed-oil paint on

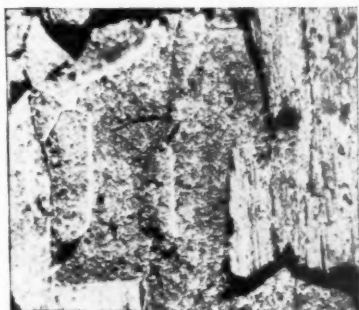


FIG. 5.—Zinc-Oxide and Linseed-Oil Paint on Wood Exposed 15 Months.

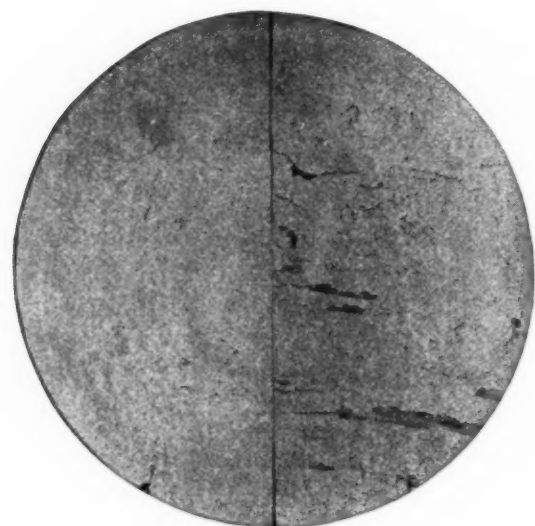
wood. It was a street sign with lampblack letters over white lead. The time of exposure is unknown; but it was sufficient to cause the wearing away of, not only the lead paint, but also part of the wood so that the letters stand out in bas-relief.



FIG. 6.—Carbon and Linseed-Oil Paint on Wood.

The slides shown have illustrated paints that, according to our waterproofing tests, varied in permeability due to a difference in their pigments. Such variation may also be brought about by keeping the pigments the same in the different paints and changing the liquid portion.

Mr. Kohr. The next slide (Fig. 7) shows the relative permeability and relative durability of two paints, one made from zinc oxide and boiled oil, and the other from zinc oxide and a waterproofing liquid. Under water the boiled-oil paint turned white in eight hours, and the other one did not turn white in six months; the difference in durability is very apparent.



(a) Zinc Oxide in Boiled Oil. Out $7\frac{1}{2}$ Months.

(b) Zinc Oxide in Special Waterproof Liquid. Out $7\frac{1}{2}$ Months.

FIG. 7.—Relative Permeability and Relative Durability of Two Paints.

The next slide (Fig. 8) serves as a further illustration of this way of modifying the waterproofing properties of a paint. The steel panel was coated with three coats of an iron-oxide paint ground in a waterproofing liquid, and was exposed for $5\frac{1}{2}$ years. The rusting can be plainly seen.

The next slide (Fig. 9) shows a paint made from the same character of pigment, but ground in linseed oil, and exposed

for the same time ($5\frac{1}{2}$ years) on steel, illustrating much greater durability of this paint, which is more permeable.

The next slide (Fig. 10) which exhibits the same paint on

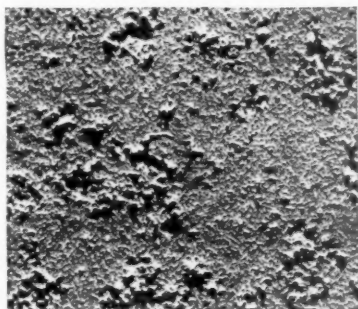


FIG. 8.—Failure of a Relatively Waterproof Paint Exposed on Steel for $5\frac{1}{2}$ Years.

wood—the two paints last shown were on steel—shows a similar result after an exposure for five years.

One or two points that might be brought out in this connection are that, while the permeability of a paint is undoubtedly

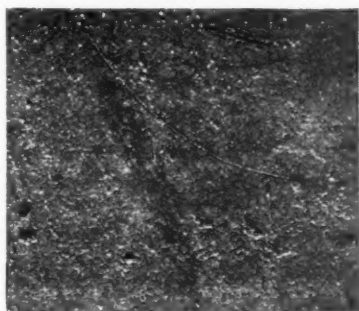


FIG. 9.—Durability of a Relatively Permeable Paint Exposed for $5\frac{1}{2}$ Years.

important in its bearing on durability, there are other properties just as important as permeability, or even durability. For example, the cost of a paint is important, or is so considered by most of us. The working properties also are very important.

Mr. Kohr. If the paint does not work properly, it will not be used by the painters, no matter how durable it is; and if it should be used, more time and expense will be required in its application.



FIG. 10.—Iron-Oxide and Linseed-Oil
Paint on Wood.

Some other properties of paint, which might be called negative properties because they are to be avoided, can be illustrated by slides.

- If a paint wrinkles as seen in the next slide (Fig. 11), which

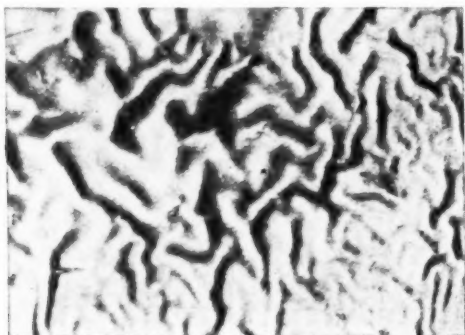


FIG. 11.—Wrinkling.

defect is more commonly found in varnish paints, it is unsatisfactory. This is also true if it sags (Fig. 12), or if it runs (Fig. 13), which in this case took place in a paint that contained excellent ingredients, but not quite in the right proportion.

Another point that is very important, having as much influence as has permeability, is flowing: the tendency of a paint film, before it has dried, to level to a uniformly smooth surface so that the brush marks do not show. The paint shown in the next

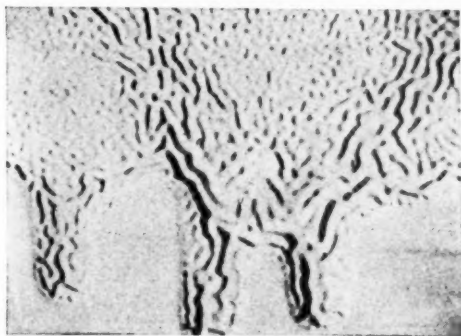


FIG. 12.—Sagging.

slide (Fig. 14) does not flow out properly. The brush marks remain after the paint has dried. On the other hand the paint shown in the following slide (Fig. 15), a paint of the same general

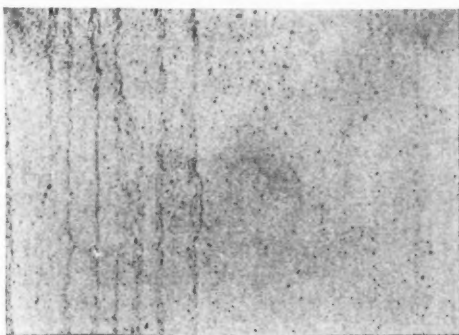


FIG. 13.—Running.

composition and made for the same purpose, shows no brush marks; it flows out properly.

The next slide (Fig. 16) shows a poorly flowing paint; the magnification is greater, and the brush marks are therefore

Mr. Kohr. more pronounced. The film shows "hills," where the paint is thick and has the maximum durability, and "valleys," where it is very thin and will soon fail.

These few illustrations merely touch upon that phase of

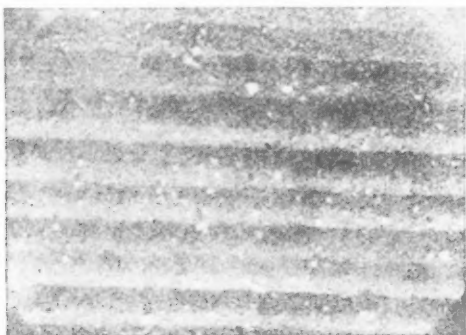


FIG. 14.—Poor Flowing.

paint technology in which the master painter is much more at home than the technically trained man.

Considering another phase of Mr. Muckenfuss' paper, I offer a criticism for which I am unable to suggest a remedy,



FIG. 15.—Good Flowing.

but he asked me to bring it up nevertheless. In his test of paints for steel the paints were applied to a wire-gauze medium, which, of course, was full of pores, and it was through these pores that the penetration took place. The next slide (Fig. 17) shows an

exaggerated cross-section of one of these, the marks illustrating **Mr. Kohr.** the medium on which the paint was applied, and the continuous line over them, the film of the paint. Here any penetration that takes place will have to pass through the film and come out at those points where the film is not in contact with the surface;

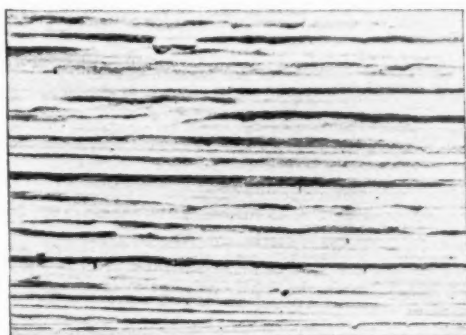


FIG. 16.—Brush Marks.

so that, as a matter of fact, the paint is tested as an unattached film, and not as a coating on a surface. From this you see that the portion of the film tested, is on no surface at all. What should be a priming coat is really a finishing coat, because it is in contact, not with the surface, but with the air. That the



FIG. 17.—Exaggerated Cross-Section of Paint Film and Wire Gauze.

surface on which a paint is applied has a marked influence on the durability of the paint, and that the priming coat is also of great importance in its influence on durability, can be readily shown.

The next slide (Fig. 18) shows two panels of a test, which have been exposed for the same length of time. The poor one has on it three coats of paint; the other, two coats of the same paint over a suitable primer, illustrating the necessity of having all of the paint film in contact with a surface properly primed,

Mr. Kohr.

and showing the desirability of bringing this about, if it can be done, in the test of Mr. Muckenfuss.

In comparing the results of these and other experiments with the work of Mr. Muckenfuss, we are led to the conclusion that he has not developed his line of investigation far enough to warrant our drawing from his results, at this time, any generalizations, or definite conclusions.

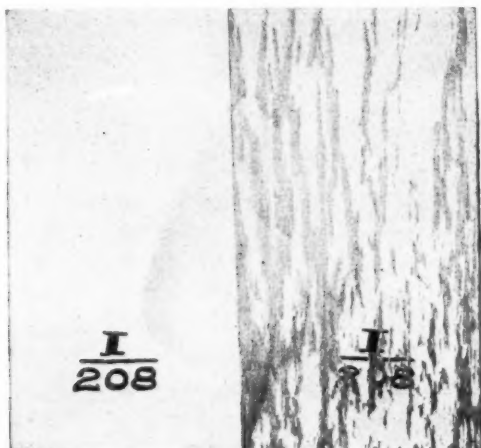


FIG. 18.—Illustrating Effect of Surface and the Importance of Proper Priming.

Mr. Thompson.

MR. G. W. THOMPSON.—I must confess that after reading Mr. Muckenfuss' very valuable paper, I feel qualified to discuss it only from a more or less academic or theoretical standpoint. The value of permeability tests is limited by the caution with which such tests may be used. In other words, permeability is but one quality and the danger arises in attempting to use such a test as a philosopher's stone, that is, as a conclusive test. I do not know whether we all realize this danger, and I can only illustrate it by referring to cases wherein more or less positive conclusions have, in the past, been drawn from certain kinds of tests without considering the relation of these tests to others and without considering the relation of the qualities brought out by these tests to other qualities. There was a time when a

great many tests were made on the tensile strength of paint films, and conclusions were drawn with regard to such tests. I will not say that this was done in this Society, but conclusions have been drawn with regard to such tests which, in practice, were found to be utterly untrue. So, too, a number of years ago, I suggested a water test with regard to the influence of pigments themselves upon corrosion. I do not hesitate to express my opinion with regard to that test. It is a valuable test for the information it gives, and should be taken in connection with other information; but when taken by itself and used by itself, it is a most dangerous thing. We have our Atlantic City test fence which was designed for the purpose of trying to bring out some relation between the water test which I suggested and the practical value of pigments in oil as protective coatings. I do not hesitate to express my opinion that this test has shown no such relation. That does not interfere with the value of the water test. It is very suggestive and it has advanced us materially, but it shows the danger of taking a single test of that kind and drawing sweeping conclusions and incidentally, I may add, doing great harm to the users and venders of various pigments. Now, in the case of this permeability test, I see the same danger and we must guard against it. It must be considered as simply giving information which must be used in connection with other information, to give us proper conceptions of what takes place in the aging of paint.

I think Mr. Muckenfuss has pointed out in these tests certain things which, if true, are very valuable. I think the results which he has given us should be confirmed by other investigators. He has shown, for instance, in one table, or diagram, that the variation in permeability cannot be accounted for by the existence of pores in the paint film. Now, permeability through pores in a paint film must be infinitely greater than permeability through the film itself; and if, by his test, it is found that permeability does not vary in a line indicating porosity, then porosity itself becomes almost a negligible factor in paint permeability. As I have said, I think this should be confirmed. Personally I am quite sure that the permeability of a paint film does not depend upon pores. You might find pores in one-coat work, but you will not find it in three-coat work. Now, if per-

Mr. Thompson. meability is a function of the solubility of water vapor in the film and its passage through the film by some system of osmosis or diffusion, we have a new conception with regard to permeability. I think that Mr. Muckenfuss' paper indicates that that is what takes place. Now here is the value of that conception: it is not that certain paints are more permeable than others—that is not the main thing; but if one paint is more permeable than another by that test, how will you proceed to modify the more permeable paint to secure less permeability, if that is desirable? Suppose we have a series of tests with regard to a certain number of pigments and it is shown that the paints made from these pigments differ very considerably in their volume permeability; that does not condemn a given pigment, but simply means that the paint manufacturer or the paint student or the man who draws up specifications will so modify the composition of that paint, either by the admixture of other pigments or by the proportioning of the materials, to get the impermeability desired.

Another very important point brought out by Mr. Muckenfuss' paper is that with most paints the permeability goes down for a long period of time. That, to my mind, is quite remarkable. His tests are exceedingly severe. I should consider 100 days in his test to equal about three years of an ordinary exposure, because in the test the conditions are alternating from day to day: a spray and exposure to the ultra-violet light and to the ozone that comes from that light. He used the bright light that is obtained from a Cooper-Hewitt quartz-tube. If, in 100 days, certain paints have decreased in permeability, it is a remarkable showing as to results. Certain other paints reach the breaking-down point so quickly, as Mr. Muckenfuss has said, that he could not determine that point; that is, the permeability increased so rapidly that it appears almost as a straight line from the initial permeability. That was shown particularly in the asphaltum paints. Now here again I feel the importance of my view so greatly that I do not, by any means, want any one to think that that test condemns asphaltum paint. It is for the paint manufacturer to determine, if impermeability is a desirable quality, how asphaltum paints can be made so that they will not break down so quickly, so that their permeability will not increase

with the great rapidity shown in these tests. The same thing is true in regard to rosin. Most of the rosin paints lost their impermeability very rapidly. It may possibly be that that defect in the particular paints which he tested is not a general defect, but a defect that can be corrected by the proper admixture of other materials or the proper preparation of the paint. Mr. Thompson.

I think this is a very valuable paper, opening up a great field of thought and giving us a new conception with regard to paint subjects. It will help us to think better on this subject, and I, for one, want to express my thanks to Mr. Muckenfuss for his valuable contribution.

MR. PETER FIREMAN.—I have not had the opportunity to read the paper, and I can therefore state only what occurs to me on the spur of the moment. There seems to be one observation I can make that may be of interest. It has been noted here that for a time the permeability diminishes, and then increases. In some cases, the breaking down begins to take place almost at once. That may have some connection with the chemical changes taking place in the pigment, in the vehicle, or in both. We know, for instance, that linseed oil undergoes certain chemical changes; that with some pigments, additional chemical changes will take place, due to the liability of the pigment itself to undergo certain chemical changes. On the other hand, we know, for instance, that rosin or gums are not likely to undergo chemical changes to any extent. It appears to me to be interesting, therefore, to try to find out the relationship between the progress of permeability and the chemical changes which are likely to occur in linseed oil, or in pigments, or in both. Mr. Fireman.

In studying the permeability of linseed oil coatings, it would be interesting also to determine how the permeability is affected by different consistencies of linseed oil that are obtained by heating the linseed oil to various degrees of consistency.

MR. F. P. CHEESMAN.—I should like to ask Mr. Muckenfuss a question. In giving the composition of various paints tested, I notice he used for test an asphaltum paint which contained 10 per cent of linseed oil and 58 per cent of benzine. Under these conditions I can readily see how an asphaltum paint would fail, as he had one of the cheapest grades of asphaltum manufactured, and this is used in comparison with a car- Mr. Cheesman.

Mr. Cheesman. bon black paint containing 82 per cent of linseed oil and no benzine. I should also like to know why he used a graphite paint containing a large percentage of benzine. Will he please inform us why he did not secure a sample of the best paint in each class, which would certainly have been the fairest way to secure correct results?

Mr. Voorhees. **MR. S. S. VOORHEES.**—There is just one point about which I should like to question Mr. Muckenfuss. Are the curves he reports the results of single determinations or are they average figures?

Mr. Muckenfuss. **MR. A. M. MUCKENFUSS.**—This preprint was not ready in time for general distribution before the convention, and as the paper consists of 67 pages with 44 illustrations, it would be difficult for any one at this date to have in mind all the points developed. I appreciate highly both the commendations and the criticisms. We are all seeking the truth for the good of both the producer and the consumer and welcome it. You will find that practically every question or criticism brought forward by the preceding speakers has been considered in the body of the paper itself or in the preliminary report. Permit me now to take up each of these briefly.

One thing emphasized as much as any other point in my two papers is that no measurement of any property of the rapidly changing paint or varnish coating is of much value unless the conditions for that measurement are carefully standardized. For example, even when soaking coated wooden panels of the same age and kind of exposure in water of the same temperature at the same depth for the same length of time, there will be more or less variation in results, due to the varying affinity of the individual pieces of wood for water. But unless we adopt a standard temperature, etc., for soaking the panels, the successive results will certainly diverge widely. This is not said in criticism of the excellent results presented, but rather by way of emphasis; for undoubtedly proper precautions were exercised.

In the case of the zinc-oxide and carbon pigments compared by Mr. Kohr, while the former started with a lower permeability, it is not shown to have maintained this low figure. The difference in the color of these pigments would indicate a

corresponding difference in chemical changes during exposure to light. The same is true of the waterproofing liquids, compared with the pure linseed oil as binders. In the former case, that of the waterproofing, the effect of water and other elements of exposure was to hasten chemical change, resulting in an increased permeability. Personally, I do not believe that the corrosion illustrated commenced until the permeability or porosity increased, though it may yet be true that high permeability spells durability without vitiating the results presented by me. Mr. Muckenfuss.

I agree very heartily with Messrs. Kohr and Thompson that permeability is only one of the properties of the protective coating, as you will see emphasized in both of my papers. The properties enumerated by Mr. Kohr, and others not enumerated, are very important. But the change in any one of them during exposure is probably indicative of the rate of change in all the others, as explained elsewhere. At the same time, I am inclined to think that permeability is the most important property of the coating, because, in proportion to the amount of water that is passing through the film, in the same proportion will that film and the surface covered decay, if solutes pass through also. It seems to me that water is the vehicle of chemical change; it is the dissociant; it hastens chemical reactions, being generally essential to them.

Moreover, I see no reason why these changes in permeability during exposure cannot be checked up against test fence results and practical experience, as suggested in the discussions, after which diagrams such as are given in my two papers could be relied upon as a criterion of durability, provided repeatable comparisons can be obtained.

One slide shown by Mr. Kohr is intended to illustrate that the steel wire cloth which I used was not a perfect imitation of the actual steel surface. This is true; for in the test under consideration the surface must be porous. Yet his illustration was not accurate because the paint on the special panels completely covers the metal as in practice, whereas his slide shows the under surface of the metal exposed. In my opinion, his objection that the moisture passes through the pores of the wire cloth and not through the steel itself is not a vital one,

Mr. Muckenfuss. though true; for when the metal corrodes, moisture will pass between it and the coating, increasing the permeability in the test, and besides, all coatings are treated exactly alike, "what is sauce for the goose being sauce for the gander." In both papers of this investigation attention is called to the need of a better imitation of the surface covered, which can be attained by further experiment.

Mr. Kohr's point in regard to the importance of the primer is timely. Of the diagrams shown by me, only five were for cases where priming coats had been applied. The field for this investigation is so vast that I simply have not had time thus far to study fully the effect of priming. There is no difficulty at all about applying such coats to the panels used by me as easily as to steel or wood panels. I certainly believe that by the use of undercoats, each pigment can be made to show up much better, but the object so far has not been to make the best showing for each pigment or coating, but rather to make comparisons as simply and quickly as possible.

In using tensile strength as a criterion of durability, which is so mentioned by Mr. Thompson, it is impossible to avoid injuring the film, and thus the same film could not fairly be used for successive measurements of tensile strength. Indeed it seems to me unfair to test films as such at all. The coating is bound up in fate with the covered surface, and any fair test should include both of these, the measurements being made without handling either of them. As to water tests with regard to the influence of pigments upon corrosion, as mentioned in the same connection, here we have the binder absent. The ideal test, to my way of thinking, puts pigment, binder, and protected surface together as in actual practice, and accurately measures the change in one characteristic as aging proceeds. Though partial or inaccurate accelerated tests have failed, we still should be hopeful of the future.

I do not wish the Society to think of my work as proving that paint has no mechanical pores. It simply indicates that porosity is an insufficient explanation; that some further theory must be postulated. The fresh coating may be partly porous and partly permeable, using these terms as mutually exclusive, but certainly I believe that after the paint begins to break down, the number of pores increases rapidly.

It will be a very easy matter for manufacturers, as has been suggested, to modify the initial permeabilities of their products by changing the proportion of pigment, etc., once the desired permeability has been established. The asphaltum is a case in point. The result that I have shown is unfair to asphaltum; for the mixture contained too little oil and too much benzine. At the same time, asphaltum is not strictly a pigment like carbon black, and much of it has in the past been applied to steel in an improper way. The reason why this particular formula was used was simply because there happened to be around the factory a can of asphaltum of the formula given, and it seemed desirable to run the gamut of quality in steel protectors. The same is true of silica graphite. As far as this formula is concerned, it is mentioned in the paper that no particular make was used. Mr. Muckenfuss.

It is distinctly stated that in the investigation up to the present stage suggestive results are given over a wide range rather than detailed results over a small range. Detailed results over a wide range are out of the question until the problem is attacked on a large scale. I am far from intending injury to any particular interest. I do not think that anything in this paper indicates as yet that any one pigment is necessarily better than another, when each pigment is applied under ideal conditions for its peculiarities. What is true of asphaltum is true of rosin. Formulas are shown in the two papers where rosin holds up very well. The practical thing to do is to prevent the adverse chemical change that water causes in it during exposure by the use of Chinese wood oil or other material.

Mr. Fireman brings out well the fact that these permeability changes are in fact a criterion of the chemical changes going on in the coating. He might have said physical changes also and have included the surface covered. All such alterations seem to me to be summed up in the changing of any one property, such as permeability, and thus we have a diagnosis—so to speak—of the general condition of the patient from time to time. Of course, as he says, the nature of the oil is bound to have a serious effect. I have no doubt that boiled linseed oil of various consistencies will show quite different permeabilities before and during exposure.

Mr. Muckenfuss. In regard to the question of Mr. Voorhees, let me state that the large majority of the results are duplicates which agreed very well, and that it is the average of such results which are plotted on the diagrams.

Mr. McNaughton. **MR. J. P. McNAUGHTON.**—One point has not been touched on in connection with this paper, and that is that the conditions of the tests are somewhat different from those in actual practice. In practice the film is lying against a surface and in the test it is exposed on both sides to the atmosphere and that makes a very great difference in the result if porosity is a large element in the test.

Mr. Muckenfuss. **MR. MUCKENFUSS.**—I think that this point is well taken; for, as already noted, it is impossible to have an ideal accelerated test. At the same time it must be remembered that the rims of the panels that I used were pressed against the sides of the exposure tanks. The under surfaces, therefore, were thus in contact with enclosed air during the artificial exposure, but were not affected to any extent by the artificial weather. The same is true where natural weather is used. The arrangement utilized appears to be a tolerably good imitation of the condition of the under surface of the paint in actual service.

Mr. Gardner. **MR. H. A. GARDNER** (*by letter*).—Mr. Muckenfuss' work is a most valuable contribution to paint technology. The apparatus described by him is, without doubt, the most ingenious one of its kind ever produced. In a well-equipped laboratory, concordant results could doubtless be obtained with such an apparatus. It is a question, however, whether the ordinary testing laboratory could afford such an elaborate piece of apparatus. In such event, it is probable that the simple bottle method for testing the permeability of paint films, which I described four years ago,¹ would be quite suitable. With this apparatus, the tests are made upon actual dry films of paint without any backing of fiber or other substance. Mr. Muckenfuss has found the bottle method to give results in accord with those obtained with his own apparatus.

The degree of permeability shown by paint films is a matter of considerable importance. If a paint should form a film that

¹ *Bulletin No. IX, Scientific Section, Paint Mfrs'. Assn., p. 14 (1909).*

is absolutely impermeable, it would not be suitable for application to cement or to wood. The moisture present in such structures is continually being drawn to the surface by the action of the sun, and if it should meet with an impermeable membrane of paint, this moisture would cause the dried paint to blister, and peeling and disintegration would follow. Paint should therefore be to some degree permeable to moisture, if it is to be used upon wooden or concrete structures. A paint which would dry to a film possessing valvular porosity, allowing moisture to escape from the inside but preventing the entrance of moisture from without, would, theoretically, be an ideal paint for wooden or concrete structures. It is quite possible that such a paint might be devised, as a result of further experimentation. On the other hand, a paint which would form a non-permeable film should theoretically be the ideal one for use upon iron surfaces. Mr. Gardner.

The box-method apparatus described by Mr. Muckenfuss for making accelerated tests of paints by subjecting them to strong light exposure, to water spray, etc., is well arranged, and should prove of service to large consumers of paint, provided accelerated tests give check results with actual exposures made over a long period of time. It would be well to make a series of tests with this apparatus on paint coatings which are identical in composition with those used on the wooden, cement and steel test fences at Atlantic City, Fargo, Pittsburgh, and other places, as the records of the durability of these paints are available. If Mr. Muckenfuss should find that he can secure similar results with his accelerated test apparatus, its value would at once be indicated.

MR. G. B. HECKEL (*by letter*).—Mr. Muckenfuss' work Mr. Heckel. appears to open up a promising lead for investigation. His method of intensifying service conditions is particularly interesting and may lead to the formulation of the "accelerated tests" which Committee D-1 has long sought.

The fact that paint films are not impervious to moisture has, of course, long been a familiar one. It will be remembered that the Scientific Section of the Educational Bureau, Paint Manufacturers' Association of the United States, under Mr. Robert S. Perry, some years since, published the results of a

Mr. Heckel. study of this phenomenon, and described two or three devices used in the work. It was realized then as now that the function of a paint coating being to insulate surfaces from the agencies of decay or corrosion, and these agencies being almost exclusively dependent upon water as their vehicle of conveyance, the most efficient paint would be that which would offer the greatest and the most prolonged resistance to the passage of water or water vapor.

In general, the results thus far obtained by Mr. Muckenfuss corroborate the results obtained in the Bureau laboratories. This investigator has, however, taken a very important step forward in the introduction of the accelerated time-service factor; and, this, as I have said, appears to hold promising possibilities.

If Mr. Muckenfuss' hypothesis that a measure of the change in one characteristic is an indication of the extent of alteration of the other characteristics be correct, the problem of paint testing is at once materially simplified.

The data thus far produced are, of course, too incomplete to warrant conclusions, but they appear very promising to me, and should be carried forward systematically on an enlarged scale. They should also be checked by concurrent field tests with the same materials.

Mr. Lindsay.

MR. R. W. LINDSAY (*by letter*).—The work done by Mr. Muckenfuss, as explained in this paper, has certainly been done very thoroughly and with a great deal of thought. At the same time there are one or two points which I believe have not been gone into sufficiently, or at least have not been given sufficient attention.

The first point which I should like to raise is regarding the position of the panel when the permeability is being determined; that is, as far as I can gather, in almost every test the paint or oil film has been so arranged in the testing device that the moisture passes first through the cloth paper or material over which the paint has been spread and then through the film itself. To my mind, in order to make this work more practical, it would seem to be essential that the film itself be exposed directly to the moisture, with the cloth paper or other supporting material on top; thus the moisture will first pass through the film and through the supporting material. To my mind this is very important

and should be done in all cases in order to make the test of **Mr. Lindsay** practical importance, for the reason that certain driers have the effect of causing the film to dry over the top, while the underneath part of the film is rather soft. Consequently a film of this kind would be very much more resistant when the moisture attacks the outer surface of the film at first, rather than the under part of the film, and would more nearly duplicate practical conditions.

Secondly, I should like to inquire if a series of tests has been made to establish the fact as to whether this test shows what we could expect when a film is exposed to the weather. Of course, in his paper, Mr. Muckenfuss has in a general way showed this; yet at the same time, in order to make the test positive, it would seem to me that this point would have to be very thoroughly covered. I believe that the test should be gone into very thoroughly and its value determined absolutely.

MR. A. H. SABIN (*by letter*).—Mr. Muckenfuss has shown **Mr. Sabin.** much ingenuity, skill and patience in the tests he has described, and the results are of much interest. It appears that all the films he used proved to be permeable to water vapor. This suggested some reflections to the writer, and in conference with Mr. Muckenfuss the latter expressed the opinion that they might prove suggestive to other students of the subject; hence the following brief observations.

During the years 1895-1898, the writer was carrying on a series of tests of oil and varnish paints and varnishes, on steel and aluminum plates, both in fresh and sea water.¹ It is well known that sea-water readily attacks aluminum, hence tests of protective films on that metal in sea water are conclusive one way or the other. Some of these films, in particular some of the varnish coatings, afforded practically complete protection both to steel and aluminum. It is worthy of mention that some of these varnishes, containing two to three times as much linseed oil as resinous matter, appeared soft and somewhat like very thin leather thoroughly water-soaked; they could be peeled up from the metal support in long strips, yet the underlying metal was not rusted at all, and when allowed to dry the films became hard again and regained, in part, their natural gloss or luster. There is no question as to the facts,

¹ "Technology of Paint and Varnish," pp. 220-242.

Mr. Sabin, because they were inspected on removal from the water by the U. S. Naval constructors, by several well-known engineers, and by representatives of the *Engineering News* and *Engineering Record*, who had been invited to be present; also the plates were subsequently exhibited before the American Society of Civil Engineers, the Engineers' Club of Philadelphia and the Boston Society of Engineers. Now, if these films were permeable, why did the plates not rust, especially the aluminum ones? The same is true of the plates of the Havre de Grace test. After five years' exposure to the weather nineteen of these plates, one of each kind of paint, were removed, inspected, the paints cleaned off with caustic soda, and in most cases the steel was exactly as free from rust as when it was first painted. Why did not these rust? There is no question about the facts; and no one doubts, on the other hand, that Mr. Muckenfuss did really find that similar films allow water vapor to pass through rather easily. Who will explain these apparently contradictory facts? Not the writer; but attention may be directed to the common experiments on osmosis, a subject of physical chemistry. The chemist has found that there is a class of substances, usually spoken of as films, which are semi-permeable; they will allow certain solvents, such as pure water, to pass through, but will not admit the passage of substances in solution in these solvents; so in a manner, they afford a way of separating the dissolved matter from the solvent. May it be possible that films which have proved useful for protective coatings, while they are permeable to absolutely pure water, which does not cause corrosion, do not admit the passage of ionized substances which are active in that regard? It is a fact that these semi-permeable films, as used in chemical experiments, do not allow ionized matter to pass; they are never spoken of as being porous, their permeability being restricted to certain fluids, and in fact they are artificially made on a porous foundation as a support. The water which, in Mr. Muckenfuss' experiments, passed (in vapor) through the films, may have been absolutely chemically pure water. It may be that a film which has value as a protective on metal is entirely free from porosity, having no holes in it except such as are due to mechanical imperfections; that it is, in fact, a semi-permeable membrane; and that its usefulness

terminates when, either from chemical or mechanical action, **Mr. Sabin.** or both, it loses its mechanical continuity and admits ordinary impure water and air.

MR. G. D. WHITE (*by letter*).—From a theoretical as well **Mr. White.** as a practical viewpoint, the apparatus described by Mr. Muckenfuss and his method of application for testing of protective coatings, both paint and varnish, are feasible and commendable. Mr. Muckenfuss has worked long and patiently to develop an accelerated test, and his achievement, it seems to me, marks another step forward in paint science.

The work covers a broad field, and while the results obtained so far do not prove positively the reliability of his testing method, nor that those results will always be in concordance, they demonstrate a new and valuable procedure for establishing paint values. When the work has been carried to completion, it is my firm conviction that comparative results will be concordant and that reliable data on the merits of various protective coatings can be procured.

Allow me to add a word of caution and emphasize the absolute necessity of extreme care in dealing with a question so important as the one under discussion. Deductions will not be reliable unless the paint films are without variance in uniformity, and it is only experience that teaches how difficult it is to obtain that uniformity. No ordinary laboratory where chemists are crowded for time in performing their many duties is in position to justify an attempt to follow the procedure outlined by the author. In a laboratory favorably equipped, results of value cannot be realized if the work is done by an unscrupulous, careless, or inefficient operator.

The experimental work as carried on by Mr. Muckenfuss and his assistants has been most elaborate in the attempt to duplicate natural weathering agencies, and practically every important factor has been scientifically considered. So manipulated, I repeat, the apparatus should be most valuable for the testing of films in determining their adaptability to withstand abnormal agencies as well as ordinary weather conditions.

Practically all the work that has been done heretofore along the lines of accelerated tests, has been the subjection of paint films to rapidly disintegrating influences, and the results as

Mr. White.

tabulated are a measure of the permeability at different intervals. The measurement of an electrical current in various stages of the test has served as a base for the computation of results by a number of investigators. The trouble with a majority of these tests is, in my opinion, that they have been performed in solutions or under conditions that must be considered not only abnormal but absent in practice, and consequently I cannot see how the data obtained are of much value, even comparatively speaking.

As shown in charts exhibited by Mr. Muckenfuss, the paints having a low initial permeability are not necessarily, and rarely are, the most durable when employed for different purposes; but for known uses, the initial permeability of the paint film should be valuable information for an experienced operator. The service conditions, however, must determine whether the film should initially have a low, medium, or high permeability.

I am convinced that the measure of permeability determined at different times while the film is passing through the various stages of metamorphosis is the best criterion we have of durability and efficiency. It is a generally recognized fact that as a paint or varnish film deteriorates, it becomes more and more permeable to agencies that have a deleterious effect on the surface it is designed to protect. This being true, the measure of the permeability initially and at different periods thereafter, will be a measure of the efficiency of the paint or varnish coating, providing the conditions to which it is subjected will simply be an acceleration of those to which the particular coating would be exposed in actual practice.

For example, a mill white, if submitted to the same exposure or test as a bridge paint, would prove an absolute failure, and yet be the very best paint possible to devise for the purpose intended.

Mr. Muckenfuss.

MR. MUCKENFUSS (*Author's closure, by letter*).—Before taking up the discussions "by letter," let me refer briefly to three other points. While regulation panel tests upon the roof, as illustrated by Mr. Kohr, are the chief reliance of manufacturers and are at present our most acceptable criteria of the quality of protective coatings, those tests have, as is well known, many serious disadvantages, such as slowness, dependence upon vary-

ing individual judgments, difficulty of recording, etc. Even Mr. Muckenfuss, then, the application of the coating to an actual structure will frequently give unexpected results.

During this discussion, one has often heard the expression, "more permeable." I desire to call your attention again to the need for a definition of this term. When two different liquids, A and B, are spread side by side and tested and when A is found to have an actual permeability greater than B, does this mean that it is necessarily really greater? The coating of B might have been thicker or heavier. Such data must be considered before we can compare the two films.

I do not think that any points have been developed during the discussion, either orally or by letter, that are inconsistent with the results presented by me. This is a comparatively new field and the amount of work demanded is enormous; hence we cannot yet conclusively explain the meaning of all the facts at present known.

Turning now to the discussions by letter, it is noticed that two refer to the apparatus and method used as being too complicated and expensive for the ordinary commercial laboratory. This may be to an extent true of some of the methods for artificial exposure, but it is certainly not applicable to the device for measuring permeability. The latter can be made inexpensively of smaller size, out of materials obtainable from the local foundry, tin-shop, and dealer in glass ware, and the tests can be performed in an ordinary air bath having temperature regulation. Of course, the greater the care that is exercised, the greater will be the accuracy and reliability of the results, just as in any other test. Even the larger outfit as utilized by me was not very costly. If one wishes to measure the permeability of coatings during exposure past the breaking-down point, some such apparatus, where the films are not handled, seems unavoidable.

Undoubtedly, as several have pointed out, paints for many surfaces should be permeable. There is what might be called an "optimum permeability" for each surface, above or below which less protection is afforded. Hence, when the permeability gets down below this hypothetical point, actual pores commence to appear and the amount of water passing through the film increases.

Mr. Muckenfuss.

Replying to Mr. Lindsay's two questions, I desire first to agree with him that the position of the panel on the testing device is important. The "double panels"¹ described can be set in the mercury groove on either side and when so used give results as shown in Fig. 31. I believe, as he does, that the results would be more accurate if the painted side of the panel were next to the water in the pan during a measurement, and this can easily be accomplished. However, as this source of error did not seem to be large, since all tests were always made the same way, it was thought best to coat the panels on the outer side, where smooth films were more readily brushed out.

The second question, as I understand it, refers to the effect on permeability of exposing the coated panels of the apparatus to ordinary weather. Not much work has been done along that line, this being, as stated, the next stage of the investigation, but enough has been done² to show that permeability will decrease to the breaking-down point and then increase, just as with artificial exposure.

Mr. Sabin's explanation of the preservation of aluminum plates in sea water by coats of oil varnishes seems to me to point to the most important factor involved. We shall apparently be forced to consider the permeability of the protective coating to be a phenomenon of osmosis as Mr. Thompson also puts it. Further, in the case cited, was not the varnish prevented from continuous oxidation and decay by the sea water, and did it not perhaps retain inhibitive vapors that otherwise would have passed into the air? I believe that if the varnish were spread on aluminum-wire cloth upon one of the special panels, immersion in sea water would not much change its permeability. In other words, constancy in permeability is a measure of the degree of protection.

As to uniformity of films, this is an important practical detail. I am not a master painter, and wish that I had the skill of such an expert in applying the coats. I used the best brushes obtainable. But the panels were about 8 in. in diameter, which gives plenty of room to counterbalance any unevenness of film. You will find that for this reason duplicate

¹ See Fig. 1, Plate III, opposite p. 362.

² See Fig. 36, p. 411.

panels agree very well both in initial and in later permeability measurements. **Mr. Muckenfuss.**

Mr. White's comparison of a mill white with a bridge paint under conditions of exposure for the latter, calls attention to the fact that the artificial exposure used, if used at all, should be of the kind that the coating is designed to withstand. In my work so far, not much variation of the kind of exposure has been attempted, but it would not be difficult to imitate any climatic or other condition that the coating should meet and ascertain at regular intervals the effect of such on permeability.

EXAMINATION OF CHINESE WOOD OIL.

BY E. E. WARE AND C. L. SCHUMANN.

SUMMARY.

This paper deals with the development of two methods for the quantitative estimation of the adulteration of Chinese wood oil.

The principle upon which the first method is founded is the formation, in wood oil, of an isomeric glyceride that is insoluble in light petroleum ether. This reaction ordinarily takes place very slowly even in bright sunlight, but it may be accelerated by the addition of any one of a number of materials in small quantity. The precipitate may be filtered out, and the residue weighed after freeing from petroleum ether. This residue consists of the adulterant and the unaffected glyceride of the wood oil. The weight of the unaffected glyceride may be taken as 7 per cent of the weight of wood oil present.

The second method depends upon the insolubility in alcohol of the potassium soap of elaeomargaric acid. The precipitate may be filtered out and weighed, after drying in a non-oxidizing atmosphere.

The two methods show a degree of accuracy much better than the existing methods, and permit a fairly rapid estimation of the purity of samples of wood oil.

EXAMINATION OF CHINESE WOOD OIL.

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INTRODUCTION.

The use of Chinese wood oil as a varnish oil, although of fairly recent adoption, except in the Orient, has increased rapidly until at the present time wood oil has come to be considered the important oil of the varnish industry.

The value of the oil has been said to lie in its ability to form actual combinations with the abietic acid of the rosin with which it is cooked. This statement may be open to question, but the fact remains that varnishes of superior quality can be made from wood oil and rosin when they are properly manipulated.

Most of the Chinese wood oil imported into the United States is gathered in small quantities throughout the rural districts of western China, and after passing through the hands of several native collectors and dealers, reaches Hankow, where it is put into the export packages. The American buyer, although reasonably certain that he receives the oil as packed at Hankow, has no assurance that the oil he buys is representative of the oil as pressed from the nut. The wide variations that the varnish maker finds in the oil as he uses it may be due to variations in the nut and in its treatment, or to adulteration by one or more of the various middlemen during its open-basket travel from interior China to the forwarder's warehouse at Hankow.

METHODS OF EXAMINATION.

Numerous methods have been proposed for the determination of the relative purity of wood-oil samples. Polymerization of the oil by heating at a definite temperature for a definite length of time, various modifications of which treatment have been proposed by Bacon, Worstall, Potsdamer, Browne, and others,¹ is the method in most general use at the present time. This method seems to have given the most satisfaction,

¹ Boughton, "Testing of Chinese Wood Oil," *Proceedings, Am. Soc. Test. Mats.*, Vol. XIII, p. 923 (1913).

since the analytical constants of wood oil vary within rather wide limits, and this variation does not seem to be accompanied by a corresponding variation in the working qualities of the oil, as judged by its action in the varnish kettle.

However, many buyers prefer to depend upon the analytical constants in passing judgment on wood oil, claiming that the personal equation has too strong an influence on the results obtained by the heat polymerization method. The iodine number and the refractive index¹ seem to be the most reliable constants to use.

McIlhiney² offers a method for the examination of this oil that promises well when standardized. He precipitates the insoluble iodine addition products formed when wood oil is treated with iodine in acetic-acid solution, and weighs the oil remaining after evaporation of the solvent from the filtrate.

LIGHT BREAK.

A familiar characteristic that seems to have been neglected in the consideration of methods for judging the purity of wood-oil samples, is the light "break." It is a well-known fact that wood oil exposed to the light in bulk soon exhibits a flocculent white precipitate, which continually increases in amount until after some months, the oil seems to be a solid white mass. This change in wood oil upon exposure to light is reported by Normann³ to be a polymerization. Fahrion⁴ does not agree with this view but calls the change a molecular transformation. Normann also finds that light has a similar effect upon the potash soap of Chinese wood oil and that the transformation takes place more rapidly than with the oil.

This break caused by sunlight seems to be characteristic of wood oil alone, and seems to be capable of standardization to the point that permits its use as an analytical method for the estimation of the purity of wood-oil samples.

The authors in studying this phenomenon have made use of certain catalytic agents that markedly influence the rate of transformation. Among these are iodine, sulfur,⁵ hydriodic

¹ Wise, *Journal of Industrial and Engineering Chemistry*, Vol. 4, p. 497 (1912).

² *Journal of Industrial and Engineering Chemistry*, Vol. 4, p. 496 (1912).

³ *Chemiker Zeitung*, Vol. 31, p. 186 (1907).

⁴ *Chemisches Centralblatt*, Vol. 83, p. 2154 (1912).

⁵ Maquenne, *Comptes Rendus*, Vol. 135, p. 696 (1902).

acid, sulfur chloride, carbon dioxide, hydrogen sulfide, phosphorus tribromide, and carbon bisulfide.¹ These agents act at different rates, and bring to completion in times varying from a few hours to several weeks, a reaction that Morrell² reports as proceeding only to the extent of 6 per cent in one year. Sulfur chloride and iodine are the only accelerators studied, that will cause a complete precipitation within the time that permits the operation to be classed as an analytical method. Others of the catalyzers might be used as a means of getting a less contaminated product for the examination of its characteristics.

The action of sulfur chloride upon the drying and semi-drying oils is common knowledge. Jenkins³ reports the fact that sulfur chloride added to wood oil to the extent of 20 per cent will yield a jelly-like mass. The reaction is accompanied by an evolution of hydrochloric acid and considerable heat. Chinese wood oil is not alone in exhibiting this characteristic, some of the semi-drying oils showing quite as energetic an action toward sulfur chloride. The fact that such compounds are formed may not therefore be accepted as a test for the purity of wood oil. Gardner⁴ suggests that as Chinese wood oil in carbon-tetrachloride solution will form this addition product much more rapidly than will other oils under the same conditions, the action might be susceptible to a time standardization.

If the wood oil be thinned with a solvent upon which the sulfur chloride will not act, and the dilution be to a strength of solution of 20 per cent or less, the sulfur chloride then becomes merely a catalytic agent for the transformation of the wood oil by light, and there seems to be very little if any addition product formed. The oil will break to a considerable extent even in the dark, but the reaction is very much more rapid if exposed to light. If the concentration of the sulfur chloride be low and the mass cool, the reaction will proceed until within a few hours the oil is of the consistency of lard. In order that the reaction proceed to completion it is necessary that the break be filtered out at intervals.

¹ Andes, "Iron Corrosion and Anti-Corrosive Paints," p. 156.

² *Transactions, Chem. Soc.*, Vol. 101, p. 2082 (1912).

³ *Journal, Soc. Chem. Ind.*, Vol. 16, p. 195 (1897).

⁴ *Proceedings, Am. Soc. Test. Mats.*, Vol. XIII, p. 946 (1913).

The reaction between Chinese wood oil and iodine has been used as a qualitative¹, and the modification introduced by McIlhiney as a quantitative method for the estimation of the purity of wood oil. If we carry the thinning still further than is recommended by McIlhiney, and use petroleum ether instead of glacial acetic acid, the addition of 0.3 per cent of iodine will give practically no additional products, but will accelerate the transformation of the oil into the insoluble elaeostearic glyceride of Fahrion.

Analytical Method No. 1.—The authors' procedure is as follows: Five grams of the oil to be examined are weighed into a small beaker, thinned with 25 cc. of petroleum ether 60 c., b. p., and the whole cooled to 0° C. After cooling, 5 cc. of ice-cold, saturated solution of iodine in petroleum ether are added with stirring. Precipitation starts within a few minutes; the better the light the quicker the precipitation. After about 1 hour, more petroleum ether is added and the mass stirred thoroughly. The first filtration should take place after about 3 hours, the precipitation having proceeded by that time to show a conversion of about 50 per cent of the wood oil present. Little is gained by waiting longer before filtering, as the precipitate is so heavy as to mask the further action of light upon the oil. The filtrate from the first filtration is cooled and again exposed to light. A few milligrams of iodine in solution may be added at this point if the solution has become colorless. Three precipitations with the corresponding filtrations are generally sufficient to give total yield. The solution should be kept cold at all times during precipitation, although after each filtration the extra petroleum ether may be evaporated on the steam bath, if the solution be thoroughly cooled before the further addition of such and the exposure to light.

The material taken out by the third filtration is of somewhat different character from that precipitated earlier in the operation. This may indicate the presence of a third glyceride as a constituent of wood oil, and may account for the difference of opinion between Cloez,² who claims 75 per cent elaeomargaric

¹ Boughton, *Journal, Soc. Chem. Ind.*, Vol. 28, p. 719 (1909).

² *Comptes Rendus*, Vol. 81, p. 469 (1875).

acid and 25 per cent oleic acid, and Fahrion¹ who finds 90 per cent and 10 per cent.

In Table I are compiled the data obtained by the use of this method in the examination of several Chinese wood oils and of other samples in which these oils were mixed with varying amounts of sesame and soya bean oil. The figures in the column entitled "Weight of Adulterant (calculated)" were obtained as the difference between the actual weight of the residue and the calculated weight, using 7 per cent of the weight of the wood oil

TABLE I.—EXAMINATION BY PRECIPITATION OF LIGHT BREAK.

Sam- ple No.	Weight of Sample, g.	Weight of Adul- terant, g.	Kind of Adulterant.	Weight of Resi- due, g.	Wood Oil Precipi- tated (calcu- lated), per cent.	Weight of Residue (calcu- lated as 7 per cent), g.	Weight of Adul- terant (calcu- lated), g.	Adul- terant present, per cent.	Adul- terant found, per cent.
1	5.000	0.365	92.7	0.350	0.015	0.000	0.3
1	5.035	0.415	91.8	0.353	0.062	0.000	1.2
1	5.440	1.010	Sesame	1.335	94.0	0.381	0.954	15.7	14.8
1	5.160	1.000	Sesame	1.400	92.2	0.361	1.039	16.2	16.9
1	5.040	0.580	Sesame	0.920	93.3	0.353	0.567	10.3	10.1
1	5.120	0.500	Soya bean	0.815	93.8	0.358	0.457	8.9	8.2
1	5.030	1.040	Soya bean	1.400	92.8	0.352	1.048	17.2	17.3
1	5.010	1.010	Soya bean	1.330	93.6	0.357	0.973	16.8	16.1
1	4.945	1.665	Soya bean	1.920	94.8	0.346	1.574	25.2	23.8
1	6.680	0.420	Soya bean	0.940	92.2	0.468	0.472	5.9	6.6
1	5.180	0.495	Soya bean	0.825	93.6	0.363	0.462	8.7	8.1
1	5.040	0.280	Soya bean	0.650	93.1	0.353	0.297	5.3	5.6
1	6.810	1.150	Soya bean	1.470	95.3	0.477	0.993	14.5	12.5
2	5.130	0.320	93.6	0.359	-0.030	0.000	-0.6
2	5.135	0.360	93.0	0.360	0.000	0.000	0.0
3	5.135	0.300	94.1	0.360	-0.060	0.000	-1.1
3	5.600	0.360	93.6	0.392	-0.032	0.000	-0.6

NOTE.—Wood oil No. 1 was furnished by the Acme White Lead and Color Works, and is a Hankow oil of high quality. Wood oils Nos. 2 and 3 are the standard oils from the American Society for Testing Materials, and were furnished through the courtesy of Mr. H. A. Gardner and Mr. L. P. Nemzek.

in the sample as the average weight of residue obtained from pure oil. In cases where the figures show negative, indicating an actual weight less than the calculated, the figures were inserted merely to show the extent to which the inaccuracy of the method is apparent.

The results in Table I indicate a precipitation of approximately 93 per cent of the wood oil present, when using iodine as a catalyzer. Sulfur chloride does not seem to be so efficient in its action upon the raw oils. The precipitation proceeds at

¹ *Chemisches Centralblatt*, Vol. 83, p. 2154 (1912).

a slower rate, and there seems to be a strong tendency for the material to skin over with an oxidized film during the precipitation. No quantitative results on sulfur-chloride precipitation are included in the table.

The method of estimation used in arriving at the figures given in Table I is the weighing of the final filtrate after evaporation of the petroleum ether. The amount of adulteration may be considered as the weight of the filtrate minus the amount of the residue due to the unprecipitated olein present in the wood oil. This amount of unprecipitable matter has been taken as 7 per cent of the wood oil, that amount having been shown to be an average for a number of determinations on supposedly pure wood oil.

It is possible to get an estimation of the amount of wood oil present by the direct weighing of the precipitated matter. In that case great care must be exercised to prevent oxidation of this material during drying.

The tabulated results show the possibilities of this method as a means of detecting adulteration in wood oil. It is certain that further refinement of the method is possible, which will add to its accuracy.

INSOLUBLE SOAP.

In studying the characteristics of this solid precipitated glyceride, it was noted that the potassium soap was but slightly soluble in absolute alcohol, which agrees with the findings of Normann¹ and Morrell² who worked on the material precipitated by sunlight alone. Morrell, however, also states that the soap is practically insoluble in water, which statement is not borne out by the experience of the authors.

The comparative insolubility of the potassium soap of Chinese wood oil offers possibilities of a rapid method for the detection of adulteration in wood-oil samples. The saponified product separates quite completely from absolute alcoholic potash, and may be washed free from the soap of the adulterating material. If the method be carried out at uniform temperature, preferably 0° C., and the alcohol for washing the precipitate be previously saturated with the soap, it is possible to estimate the amount of adulteration within fairly narrow limits.

¹ *Chemiker Zeitung*, Vol. 31, p. 188 (1907).

² *Transactions, Chem. Soc.*, Vol. 101, p. 2082 (1912).

Analytical Method No. 2.—A 3-g. sample of the oil to be examined is saponified with 100 cc. of N/4 absolute alcoholic potash for $\frac{1}{2}$ hour, using an air condenser of sufficient length to prevent the loss of alcohol. The saponified mixture is cooled to 0° C., held for 10 minutes at that temperature, and filtered through a Gooch crucible using a filter-paper disk instead of an asbestos pad. The precipitate, after washing thoroughly with ice-cold saturated absolute alcohol, is removed from the crucible to a cover glass, and is dried at 75 to 80° C., under vacuum, and with a stream of dry hydrogen or carbon dioxide passing through the desiccator. After cooling without removing from the desiccator, the precipitate is taken out and weighed, and the weight calculated to wood oil.

It is necessary, if the method is to be at all accurate, to use absolute alcohol both for saponification and for washing the precipitate, as the soap is appreciably soluble in the presence of even small amounts of water. This alcohol and alcoholic potash should be freshly saturated with the soap before use, for although the soap is but slightly soluble in absolute alcohol, that solubility changes on standing, especially if exposed to light. It is a comparatively easy matter to keep freshly saturated solutions ready for use by making up a batch of the soap and introducing it into the stock bottles of alcohol and alcoholic potash, in quantities more than sufficient to saturate them at 0° C. When about to make a determination, the solution may be warmed until an appreciable amount of soap goes into solution, after which it may be cooled to 0° C. and held for 10 minutes and filtered. This fresh filtrate is ready for use, and the fact that the procedure followed will be duplicated on the saponified sample helps to insure the accuracy of the determination.

During the washing of the precipitate the whole must be kept cold, for the solubility of the material in alcohol is greatly increased by a rise in temperature. This is accomplished by

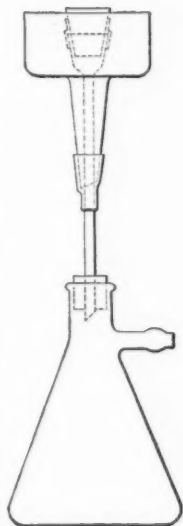


FIG. 1.

filtering through a Gooch crucible surrounded by cracked ice. The authors have found the apparatus shown in Fig. 1 to be quite satisfactory.

The precipitate is susceptible to oxidation and must be kept away from air during drying. Also, the fact that high temperature will char the soap limits the drying temperature

TABLE II.—EXAMINATION BY PRECIPITATION OF INSOLUBLE POTASSIUM SOAP.

Sample No.	Weight of Sample, g.	Weight of Adulterant, g.	Kind of Adulterant.	Weight of Soap, g.	Weight of Wood Oil (calculated), g.	Wood Oil Recovered, per cent.	Weight of Adulterant Found, g.	Adulterant Found, per cent.	Adulterant Present, per cent.
1	3.301	3.334	3.334	101.0	-0.033	-1.0
1	2.814	0.304	Linseed	2.807	2.807	99.8	0.311	9.9	9.8
1	3.012	0.180	Linseed	3.021	3.021	100.3	0.171	5.5	5.6
1	2.516	0.506	Linseed	2.508	2.508	99.8	0.514	17.0	16.7
1	3.138	3.170	3.170	101.0	-0.032	-1.0
1	2.859	0.959	Soya bean	2.874	2.874	100.6	0.944	24.7	23.8
1	2.642	0.881	Soya bean	2.645	2.645	100.1	0.878	24.9	25.0
1	3.013	3.006	3.006	99.8	0.007	0.2
1	3.006	0.167	Soya bean	2.982	2.982	99.2	0.191	6.0	5.3
1	2.051	1.302	Soya bean	2.020	2.020	98.1	1.333	39.8	38.9
1	2.461	0.125	Linseed	2.536	2.536	103.0	0.075	2.9	4.8
1	2.791	0.607	Linseed	2.883	2.883	103.3	0.515	15.2	17.9
1	2.742	0.380	Linseed	2.761	2.761	100.7	0.361	11.5	12.2
1	2.742	0.183	Linseed	2.741	2.741	100.0	0.184	6.3	6.3
1	2.528	2.501	2.501	98.9	0.027	1.1
1	2.524	2.550	2.550	101.0	-0.026	-1.0
1	2.521	2.510	2.510	99.5	0.011	0.5
1	2.510	2.514	2.514	100.2	-0.004	-0.2
1	2.321	0.707	Linseed	2.367	2.367	102.0	0.661	21.8	22.4
1	2.428	0.738	Linseed	2.508	2.508	103.3	0.658	20.8	23.3
1	3.019	3.019	3.019	100.0
1	3.039	0.409	Mixed	3.025	3.025	99.5	0.423	12.3	11.9
1	3.199	1.049	Mixed	3.201	3.201	100.3	1.038	24.5	24.7
2	3.252	3.224	3.224	99.1	0.028	0.9
3	3.471	3.544	3.544	102.1	-0.073	-2.1
4	3.156	3.014	3.014	95.5	0.142	4.5
4	3.190	3.092	3.092	96.9	0.098	3.1

NOTE.—Wood oil No. 1 was furnished by the Acme White Lead and Color Works, and is a Hankow oil of high quality. Wood oils Nos. 2 and 3 are the standard oils from the American Society for Testing Materials, and were furnished through the courtesy of Mr. H. A. Gardner and Mr. L. P. Nemzek. Wood oil No. 4 was furnished by Mr. L. P. Nemzek, representing the Educational Bureau of the Paint Manufacturers' Association. This oil is from American-grown nuts.

to about 80° C. The precipitate may be dried in a desiccator connected by a long rubber tubing to a source of hydrogen or carbon dioxide, and to a vacuum pump. After introducing the precipitate to be dried, the desiccator may be placed in a low-temperature steam drying oven for 3 hours.

The results obtained by the use of the insoluble-soap method

and the data from which these results were computed, are compiled in Table II. In calculating the results for this table it was decided to accept the weight of the dried insoluble soap as representative of the weight of wood oil present in the sample, since the error of this assumption was considered to be within the limits of error of the method.

Although neither of the above methods has proved entirely satisfactory as a method for the estimation of wood oil in rosin varnishes, the efforts made to apply them to that end have resulted in some interesting data regarding the conditions existing in this type of varnish.

An examination of the separated constituents of a Chinese-wood-oil rosin varnish seems to show that the polymerization of wood oil in the presence of rosin is practically as complete as in the oil, subject to the same heat treatment in the absence of rosin. The fatty acid in each case shows a molecular weight double that of the fatty acid of raw wood oil. The difference in characteristics between these two polymerized products has not as yet been satisfactorily accounted for.

A comparison of the molecular weights of the fatty acids from raw wood oil and the light breaks seems to show that this light break is an isomer rather than a polymer, and that there is practically no difference between the light break formed slowly by sunlight alone and that catalyzed by iodine.

DISCUSSION

Mr. Boughton.

MR. E. W. BOUGHTON (*by letter*).—The methods developed by the authors are most interesting and have given the first results of a quantitative determination of adulteration. The ingenious use of a catalytic agent to accelerate the formation of the glyceride of β -elaeostearic acid has been made the basis of a simple and satisfactorily accurate method, as described under Analytical Method No. 1.

The results obtained with Analytical Method No. 2 are also very close to the theoretical. From my experience with the salts of elaeostearic acid, which oxidize very rapidly, I should think that this method would be more difficult to perform satisfactorily than the first method. Working along the same line, I was able to obtain from the fatty acids of Chinese wood oil only a yield of potassium elaeostearate that was far below the theoretical. However, I used 95-per-cent alcohol at 3° C., and the results of Messrs. Ware and Schumann with absolute alcohol at 0° C. prove their method to be remarkably accurate for determinations of that sort. The assumption that the weight of the potassium soap obtained is equal to the weight of the Chinese wood oil present is the basis of the calculation of the amount of adulteration. As the reason for this assumption is necessary for a clear understanding of the chemistry of the method, it seems to me that the following data should be appended to the paper.

The molecular weight of the glyceride of elaeostearic acid ($C_{18}H_{31}O_2$)₃C₃H₅, is 878.6. On saponification this yields three molecules of potassium elaeostearate, $C_{18}H_{31}O_2K$, the molecular weight of which is 318.3; $318.3 \times 3 = 954.9$. The factor for calculating the amount of the glyceride from the potassium soap is therefore, $878.6 \div 954.9 = 0.92$. Chinese wood oil contains about 90 per cent of this glyceride, so that the weight of the latter divided by 0.9 gives the equivalent weight of the oil. In other words the two factors approximately neutralize each

other and the weight of the soap is equal to the weight of the Chinese wood oil taken. **Mr. Boughton.**

It is to be hoped that the authors will soon supplement this valuable paper with some data, briefly mentioned on the last page, on the analysis of varnish containing Chinese wood oil.

QUANTITATIVE DETERMINATION OF BODY AND SOLVENT IN VARNISH.

BY A. L. BROWN.

SUMMARY.

Manufacturers of electrical apparatus use large amounts of varnish, mainly for insulating. In certain applications, as in the treatment of cloth, paper, transformer iron, etc., these insulating varnishes are reduced to a lower specific gravity with benzine, turpentine substitutes, or other solvents. The covering power of a varnish depends mainly upon the amount of body which it contains. Thus it becomes necessary in a comparison of two varnishes for a specific application, to determine accurately the percentage of body in them. Of two varnishes similar in properties and the same in price, the varnish which contains the higher percentage of body is the better investment.

In a search through varnish literature, the author has found but one method for determining the percentage of body and solvent. The method consists in distilling a measured or weighed amount of varnish and noting the amount of solvent recovered. Most insulating varnishes contain, as thinners, benzine or other light petroleum oils. When distilling such a varnish, the solvent cracks up into light and heavy hydrocarbons and usually about 10 per cent (often a larger percentage) remains in the flask with the body of the varnish, and cannot be distilled even at 200° C.

The author's method in brief is as follows:

A weighed amount of the varnish is diluted with about ten times its volume of ether, chloroform, or some other volatile solvent miscible with the varnish. The solution is poured over a weighed, ground-glass plate and the solvents are evaporated at ordinary temperature in an atmosphere of illuminating gas. The added solvent, being very volatile, hastens the evaporation of the varnish solvent. The resulting film of varnish will reach constant weight in a few hours. This method gives far more accurate results than the distillation method.

QUANTITATIVE DETERMINATION OF BODY AND SOLVENT IN VARNISH.

BY A. L. BROWN.

It is a difficult matter to determine accurately the amount by weight and by volume of the body (non-volatile matter) in varnish. Most of the existing methods necessitate heating the varnish and distilling the solvent, either with or without the addition of water or steam in the distillation flask with the varnish. In all such methods part of the solvent is retained in the flask with the body of the varnish, as the heat necessary to drive off the last portion of the solvent would be so great that the other ingredients of the varnish would be altered to such an extent that any examination of them would be useless. Especially is this true with insulating varnishes, where the solvent is usually benzine or other light petroleum oils (the common turpentine substitutes). These solvents, upon being distilled from a varnish, crack up into light and heavy hydrocarbons, and the heavier portions remain in the flask even when the temperature is 180 to 200° C.

The method herein described has particular application to insulating varnishes. It should prove applicable in the case of regular varnish products made with high-grade resins and gum turpentine solvents. It is also useful in testing certain lacquers.

APPARATUS AND CHEMICALS REQUIRED.

1. Glass plates, 10 by 15 cm., and 1.5 to 3 mm. in thickness, one side of which must be ground very fine. The plates must have no broken or chipped edges.
2. Glass flasks, 50-cc. capacity, with ground-glass stoppers.
3. Pipettes, 10-cc. capacity, graduated, and accurately made, so as to deliver exactly 10 cc.
4. Ethyl ether, highest purity, containing no non-volatile

matter. (With varnishes not wholly soluble in ether, chloroform or some other appropriate solvent which is very volatile should be used. Carbon tetrachloride, tri-chlor ethylene, and other solvents of this same general class would be available. The author has found no insulating varnish where ether could not be used. Ether possesses great capillarity, spreading very evenly over a ground-glass surface, thus leaving the residual film of varnish uniformly distributed over the plate.)

5. A spirit-level for arranging the glass plates in an exactly horizontal position. (This article is not absolutely necessary, but is desirable.)

THE METHOD.

Clean, dry and weigh one of the glass plates. Clean, dry and weigh a 50-cc. glass flask with its stopper. Deliver 3 to 4 cc. of the varnish sample from a 10-cc. pipette into the flask as rapidly as possible. Stopper the flask immediately and weigh. Dilute to the mark with ether and mix contents thoroughly. By means of a spirit-level arrange the glass plate exactly horizontal. Using a clean, dry 10-cc. pipette, deliver exactly 10 cc. of the diluted varnish upon the plate, at such a rate as will gradually cover the plate entirely, but not fast enough to allow the solution to creep over the edges. A little practice will be required to do this properly. As a rule, 7 cc. may be delivered at first, the remaining 3 cc. being delivered, a few drops at a time, during the following 2 minutes.

The evaporation of the ether will carry the varnish solvent with it, and although the last portion of ether will volatilize before all the varnish solvent has disappeared, the varnish on the plate will be such an extremely thin coating that all the solvent will be gone in $1\frac{1}{2}$ to $2\frac{1}{2}$ hours. To follow the course of the evaporation, weighings may be made every half-hour.

Should the varnish contain a very active drier, place the plate, after the first half-hour, in an atmosphere of illuminating gas. Weigh every half-hour until the weight is constant, replacing the plate in the gas after each weighing. In a rapidly drying varnish, if illuminating gas is not used, the last portions of solvent will be eliminated while the body of the varnish is beginning to solidify, through absorption of oxygen from the

air. In other words, the varnish will be losing weight through evaporation of solvent and at the same time will be gaining weight through oxidation, and an incorrect determination will result.

When constant weight is obtained, the film or body of varnish on the plate will be very tacky, although the solvent is completely eliminated. From the data obtained, calculate the percentage by weight of the varnish body (non-volatile matter), subtract from 100, thereby obtaining the percentage by weight of the solvent (volatile matter).

The percentage by volume of the body and of the solvent is obtained in the following way: Distill a sample of the varnish (the author uses 100 cc. of varnish in a 200-cc. distillation flask), raising the heat gradually to not over 200° C. Many varnishes froth, filling the flask with foam and bumping violently, before the thermometer reaches 200° C. However, the object is to get as much distillate as possible without heating above this temperature. Take the specific gravity of the distillate and of the original varnish at ordinary room temperature. If the solvent is gum turpentine, distill by McIlheny's method,¹ where water is mixed with the varnish before distillation. Knowing now the specific gravity of the original varnish and of the solvent, and also the percentage by weight of the solvent, the calculations for percentage by volume of the body and of the solvent are very simple, and may be expressed as an equation as follows:

$$X = \frac{(100 A)B}{C}$$

in which A = specific gravity of varnish, $100 A$ = weight of 100 cc. of varnish, B = percentage of solvent by weight, C = specific gravity of solvent, X = percentage of solvent by volume, $100 - X$ = percentage of body by volume.

An idea of the drying qualities of the varnish film may be obtained by this method. After the solvent is eliminated and the film is constant in weight, place the glass in a free circulation of air, covering it to prevent any collection of dust particles upon it. Weigh it every hour until it begins to gain

¹ Livache and McIntosh, "Manufacture of Varnishes and Kindred Industries," Vol. II, p. 184.

in weight. The author has tested varnishes which do not begin to oxidize until 8 hours after the solvent has evaporated, while in other varnishes the film starts to oxidize after 1 hour. This test affords valuable data in the case of insulating varnishes which are to be exposed to the air; for example, in the treatment of cloth, or paper. A varnish that begins to dry almost as soon as the solvent is eliminated will invariably gum badly and cause trouble in the tanks. Conversely, a varnish which begins to oxidize only after some hours in this test, will work well in the tanks and will gum very little. Some varnish makers so load up their cloth and paper-insulating varnish with rapid driers that it becomes impossible to use them for the purpose for which they were made.

In 24 hours after the varnish is poured on the glass plate, it will have absorbed its maximum amount of oxygen. Very few insulating varnishes require a longer time to thoroughly dry. It should be remembered that the film is very thin (generally between 1 and 2 mils), owing to the great amount of dilution with the ether (3 to 4 cc. varnish and 47 to 46 cc. ether).

There are no satisfactory methods for separating the resin and oil in a dried film of varnish. Perhaps the nearest approach to accuracy is McIlheny's process, where the thoroughly dried film is weighed, broken into small pieces in an appropriate solvent which is supposed to dissolve the resin and leave the oxidized oil (linoxin) as a residue, which may be washed, dried, and weighed.¹

The object of this paper, however, is not the examination of the dried varnish film, but only the accurate determination of the solvent and body in varnish. When a consumer is using large quantities of insulating varnish, he wants to know the amount of body in the material. He is not interested in buying varnish which is mostly solvent, because he can add solvent to the varnish as required.

Two insulating varnishes cannot be compared for amount of body by determining the specific gravity. The heavier varnish may have less body than the lighter one, or it may

¹ Livache and McIntosh, "Manufacture of Varnishes and Kindred Industries," Vol. II, p. 183.

contain a heavier solvent. Also, the specific gravity may have been increased by boiling the oil for a very long time.

The author considers the accurate separation of the solvent and body in a varnish to be one of the most important tests to which the material can be submitted. It generally bears a very direct relation to the covering power of the varnish.

NOTES.

1. Instead of using a 50-cc. flask and a 10-cc. pipette, which apparatus is, of course, useful for making several duplicate tests, the author now uses a 10-cc. flask with stopper and a special pipette delivering about 0.7 cc. of varnish. Ether is added to the mark, or near the mark, on the neck of the flask and the entire contents of the flask are shaken thoroughly and poured slowly on the weighed glass plate. The flask is then rinsed with 2 or 3 cc. of ether, which is also poured on the glass plate.

2. To avoid the possible creeping of the ether solution over the edges of the glass plate, it might be an improvement to have a plate molded so that the edges are turned up a little all around the plate. The top of the plate, however, must be finely ground, as an unground, smooth glass surface will prevent the solution from spreading out evenly over the plate.

3. Some varnishes, especially certain asphaltic insulating compounds, will not remain evenly spread over a ground-glass plate, irrespective of the solvent used. When part of the solvent has evaporated, the solution will break away from the edges of the plate and collect in several spots, thus drying unevenly and making a film which is very thick in some places. It often requires 12 hours or more to eliminate the solvent in such cases. To obviate this difficulty, a molded plate (described in note No. 2, above) should be used. Fifty-mesh sea sand, previously dried and ignited, is sifted over the plate in a very thin layer, no thicker than the diameter of one of the grains. The plate should be visible between the grains of sand. The excess sand on the turned up edges of the plate should be carefully removed, the plate then weighed, and the method continued as described in the body of the paper. The sand prevents any breaking of the solution, allowing uniform evaporation of the solvent from all parts of the plate, and, provided a thin enough layer of sand

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EXAMPLES.

ALL WEIGHTS ARE GIVEN IN GRAMS.

	SAMPLE NO. 1.	SAMPLE NO. 2.
Weight of 50-cc. flask and stopper, and varnish.....	23.2805	24.9052
Weight of 50-cc. flask and stopper..	20.1715	21.4622
Weight of varnish (3.44 cc.)	3.1090	(3.83 cc.) 3.4430
Weight of varnish in 10-cc. ether solution.....	0.6218	0.6886
Weight of glass plate.....	78.9410	85.6282
Weight of glass plate and film after $\frac{1}{2}$ hour.....	79.3660	86.0800
after 1 hour.....	79.3610	86.0190
after $1\frac{1}{2}$ hours.....	79.3595	86.0136
after 2 hours.....	79.3584 ^a	86.0114
after $2\frac{1}{2}$ hours.....	79.3584	86.0108 ^a
after 3 hours.....	79.3600 ^b	86.0109
after 4 hours.....	79.3631	86.0110
after 5 hours.....	79.3664	86.0109
after 8 hours.....	79.3746	86.0108
after 9 hours.....	79.3766	86.0108
after 10 hours.....	79.3780	86.0129 ^b
after 24 hours.....	79.3900	86.0408
after 48 hours.....	79.3886	86.0392
after 96 hours.....	79.3870	86.0396
after 120 hours.....	79.3878	86.0390
^a Solvent eliminated.....	79.3584	86.0108
Weight of glass plate.....	78.9410	85.6282
Weight of body.....	0.4174	0.3826
Weight percentage of body....	$\frac{0.4174 \times 100}{0.6218} = 67.13$	$\frac{0.3826 \times 100}{0.6886} = 55.56$
Weight percentage of solvent....	$100 - 67.13 = 32.87$	$100 - 55.56 = 44.44$
Specific gravity of varnish (ordinary temperature).....	0.900	0.883
Weight of 100 cc. of varnish (ordinary temperature).....	90.00	88.30
Weight of solvent in 100 cc. of varnish.....	$\frac{32.87 \times 90}{100} = 29.58$	$\frac{44.44 \times 88.30}{100} = 39.24$
Specific gravity of solvent (distilled) (ordinary temperature).....	0.753	0.751
Weight of 1 cc. of solvent.....	0.753	0.751
Volume percentage of solvent	$29.58 \div 0.753 = 39.29$	$39.24 \div 0.751 = 52.25$
Volume percentage of body	$100 - 39.29 = 60.71$	$100 - 52.25 = 47.75$
Film at constant weight (weight $a - b$)... 1 hour		$\frac{1}{2}$ hours

^b Film beginning to oxidize.

has been used, the solvent is eliminated in 3 or 4 hours. The author does not recommend this variation of the method except for varnishes which will not remain evenly spread over a ground-glass plate.

4. The reason for using glass plates is on account of their non-hygroscopic property. The author has tried balanced filter papers, absorption blocks, alundum plates, etc., but none of these materials was satisfactory. Duplicate tests with glass plates check very well.

5. If this method is compared with the ordinary distillation process for determining the amount of solvent in insulating varnish, it will be found that in the latter method at least 10 per cent of the solvent remains in the distillation flask with the varnish, and cannot be separated from it. This statement does not apply to gum turpentine solvents.

SUMMARY.

The facts determined by this method are as follows: Percentage of the body by weight and volume, percentage of the solvent by weight and volume, relative rapidity of drying of varnish at ordinary temperature, and physical characteristics of the dried film.

Two examples are given showing a comparison of two insulating varnishes submitted for the treatment of cloth used for electrical insulation. Sample No. 1 dried so rapidly that it proved unsuitable and a practical shop test in the cloth-insulating towers was unnecessary. A laboratory test was sufficient to condemn it.

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Holley and Ladd, "Analysis of Mixed Paints, Color Pigments, and Varnishes," Chap. XIX.

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A RATIONAL TEST FOR METALLIC PROTECTIVE COATINGS.

BY J. A. CAPP.

SUMMARY.

It is proposed to test metallic protective coatings such as those applied to iron and steel by galvanizing, sherardizing, lohmanizing and other processes, by exposing the articles to an atmosphere saturated with salt-water. The saturated atmosphere is produced by projecting into the test chamber an atomized spray of water saturated with common salt in solution. Poor coatings break down in from 2 or 3 hours to 24 hours, while efficient coatings stand up at least a week. Failure is indicated by the development of red rust.

A RATIONAL TEST FOR METALLIC PROTECTIVE COATINGS.

By J. A. CAPP.

There are several processes commercially used for covering the surfaces of metals easily corroded or rusted, such as iron in its several forms, with other metals less easily corroded, or with metallic oxides. These may well be called "metallic" protective coatings in distinction from the types of coating which are in the nature of paints or their equivalent.

The object of the application of these metallic protective coatings is to enable the coated articles to resist atmospheric exposure without rusting for a longer time than they could withstand such exposure without protection. Obviously, then, the only final test of the efficiency of a given type of coating is actual exposure to the same sort of influences that the material is supposed to resist in service. If the coating is at all efficient, this takes so long a time that more rapid methods of determining relative efficiencies become a necessity. The most commonly used methods of testing such metallic protective coatings are those of chemical attack, which in effect measure either the thickness or the weight per square unit of the protective coating. Such methods of chemical attack permit the comparison of results obtained from tests upon the same sort of coating, but difficulty is encountered when attempt is made to compare the results obtained by such tests on one sort of coating with those obtained on another character of coating. For instance, the well-known Preece test yields excellent comparative results on galvanized coatings. When, however, it is used for coatings applied by the sherardizing process, the results are not at all comparable. Neither is the Preece test applicable to coatings of tin or of lead. In the case of sherardized articles, it has been suggested that the coat, which is a combined structure of zinc and zinc oxide, together with some zinc-iron alloy, be removed in strong alkalis which will not attack the iron beneath. This would enable one to determine the weight of coating per unit of surface calculated to metallic zinc, but experience has shown

that the results do not necessarily indicate the efficiency of the coat, and that it is not easy to determine the relative proportions of zinc and zinc oxide. Furthermore, comparison of the efficiency of a sherardized coating with ordinary galvanizing is not possible when the sherardized coating is tested by solution in a caustic alkali, while the galvanized coating is subjected to the Preece test.

Some years ago, when testing electrical insulation such as is used for overhead line construction, we found that material which stood fairly well when immersed in water failed badly when exposed to the weather, especially if exposed during a hard rain. This led us to produce a rain in the laboratory by sending a stream of water through an ordinary rosette such as is used with a gardener's watering can. The results were encouraging, but too severe, because the individual streams played steadily on one spot and produced erosion. Then we tried an atomizing nozzle, projecting a cloud of moisture into a chamber in which the test specimens were exposed; the results were better, but there was still a possibility of some wear if the article was directly in the path of the stream and near to the nozzle. The problem seemed to be solved when care was taken in placing the articles to keep them out of the direct path of the jet issuing from the atomizing nozzle. As experience was gained with this type of test, as applied to insulating material, it was found that what seemed to be the essential requirement was the maintaining of an atmosphere substantially saturated with moisture; and this saturated-atmosphere exposure has been one of the tests regularly applied to all insulating materials intended for out-door use since it was first worked out some fifteen years ago. It has been found to give reliable indications of the ability of insulation to resist weather, except, of course, as such ability is affected by extremes of heat and cold, erosion from the wind carrying dust particles, etc.

The problem of determining the resistance of protective coatings to weather corrosion is very similar to that of testing insulations for their weathering qualities. The conditions of exposure are the same, and hence there seemed to be no essential reason why the saturated-atmosphere test would not apply equally well to protective coatings as to insulation. Tests were begun several years ago to try out the method, and the only

fault found with it was that it was somewhat slow. Good coatings did not show any signs of breaking down after several weeks of continuous exposure to the fog; yet there was encouragement in the fact that bare metal began rusting in a few hours, and rust spots began developing on poorly protected surfaces in from a few days to a week.

The fact that more trouble is experienced with trolley-line suspensions along the seashore than with the same devices inland, led immediately to the trial of an atmosphere saturated with salt water, with astonishingly satisfactory results.

As now used, the test consists in exposing the articles in any convenient chamber into which there is projected an atomized spray of water saturated with common salt in solution, care being taken to avoid placing the test specimens directly in the path of the jet. To insure constant saturation, an excess of salt is kept in the water at the bottom of the chamber. The spray is produced by a jet of compressed air lifting the water to the nozzle, whence it is projected as a cloud. This apparatus is the common atomizer, so-called, used in the household. The chamber is necessarily not tightly sealed, but is open sufficiently to permit "breathing"; when used with an air jet, there is a slight pressure which is relieved through the breathing openings. If desired, the test may be modified by the use of a fine steam jet to raise the temperature of the atmosphere in the chamber. There is also the possibility of rendering the test atmosphere slightly acid or alkaline by suitable additions to the water in substitution for the salt. For use with plain water, the closet generally used for cement testing does very well, provided care is taken that it is so arranged as to maintain the air practically at 100 per cent relative humidity. When using salt solutions, recourse must be had to the atomizing jet to insure the development of the salt fog.

When exposed as described, articles have a very thin film of moisture over their surface, but there should be very few, if any, drops of sensible size on the objects. Obviously, the test is very searching, as all parts of the surface are exposed, and any pin holes or uncovered areas become evident. This gives one an opportunity to learn something of the efficiency of any protecting process in taking care of edges, sharp corners, porous

spots in the metal surface, etc. By noting the character of the final general break-down, a very good idea of the evenness of the coating applied may be obtained.

The method of test may be applied to bare metals as well as to those coated to prevent rusting. For this purpose, the plain saturated atmosphere is apt to be better than a salt atmosphere, because the latter may be too severe and hence make comparisons rather difficult.

The salt-spray test, as we have called it, has been used in the laboratory with which the author is connected for a number of years, and during the last four or five years it has also been used commercially as a check upon the process of sherardizing which is in use. The coated articles are exposed to the salt fog, and are examined from time to time to note their surface condition. When the coated material is iron in any of its several forms, red rust begins developing as soon as the coat breaks down. This rust may appear in small pin points which gradually extend, or it may appear generally over the surface of the article. When the coating is relatively thin and poor, rust may develop in from 2 or 3 hours to 24 hours, or longer. A better coat will last 2 or 3 days, but a well-applied coat of requisite thickness will last at least a week. If no rusting is developed in two weeks' time, it may safely be assumed that the life of the coating will be practically indefinite. These figures are based on experience with both sherardized and galvanized types of coating. Other types give results which lie in approximately the same range.

This method of testing is not offered in replacement of other methods of testing which have long been in use, especially when such tests are used solely for comparison on material treated always by the same process. It has, however, almost entirely displaced all such methods of test in our own practice, especially when comparisons are desired between processes of different character, because it is the only test which we have been able to devise which approximates practical conditions, and yet yields results within a reasonably short time. The salt-spray test is only an exaggeration of what may be expected at the seashore and differs only in degree, not in kind, from the normal conditions under which the article is intended to be used.

DISCUSSION.

MR. C. O. HARRINGTON, JR.—I do not know whether I am **Mr. Harrington.** straying too far from the subject, but I should like to ask Mr. Capp three questions.

First, in developing this rational test for protective coatings, especially for sherrardizing, was any attempt made to determine whether this salt-spray test might be used as a measure of the thickness of the coating? The company with which I am connected has occasion to use sherrardized iron parts in magnetic circuits. A variation of a few mills in the sherrardized coating has a considerable influence on the reluctance of the magnetic circuit, and we are trying to find some satisfactory means of determining the average thickness of a sherrardized coating. Micrometering may not prove a satisfactory means of doing it. I thought perhaps this salt-spray test might be used as a measure of the thickness of the sherrardized coating.

Second, Mr. Capp mentions having first used or first thought of using this salt-spray test in connection with insulations. It occurs to me that it might make a very good test for the weathering of molded or sheet insulations. I should like to learn the type of insulation on which Mr. Capp made these salt-spray tests.

Third, in case the salt-spray test should be used for insulations, would it not be advisable to apply an intermittent test instead of exposing the insulations continuously to the action of the salt spray? Might it not be advantageous to expose the insulations for a few minutes or an hour and then dry them for one or more hours? I have found that in making tests of insulation that we approximate actual weathering conditions most nearly by first soaking an insulation in water and then heating it in an oven to about 175° F. and then, after drying it out thoroughly, reapplying moisture. This treatment seems to cause an exaggerated weathering, warping and splitting insulation and causing it to peel and crack.

Mr. Capp.

MR. J. A. CAPP.—Answering the first question, I doubt very much if the salt-spray test could be applied as a means of measuring the thickness of the coating, because as the sample breaks down, the thickness is apt to increase by reason of the development of red rust on the surface. The coating is not dissolved off the surface, as the zinc is dissolved from a galvanized surface by the copper sulfate, where a breakdown is indicated by the deposit of copper.

In answer to the second question, insulations were tested, not with the salt-spray test, but with a plain, saturated atmosphere, which is quite severe enough for the purpose. If insulation is going to break down it will usually do so in twenty-four hours in a saturated atmosphere. The only reason for making the salt atmosphere, in the case of metal coatings, was to shorten the time. A week is about as long as it is possible to hold up shipments, awaiting the results of tests.

Since we have not used the salt spray for testing insulations, we have no experience upon which to base an answer to the third question. An intermittent test might work as a qualitative test. As applied to insulations, the saturated atmosphere test is only one of a great many others used. I mentioned this use as it was the first application we happened to make of such a means of testing and when sorely pressed for a way to test the comparative merits of metallic protective coatings, we had recourse to it, and later developed the salt spray as a modification.

Mr. Voorhees.

MR. S. S. VOORHEES.—It seems that this test has a wide application for a number of coated articles, modified if necessary by the character of the material to be tested. We have considered a similar test for paint coatings, feeling that a fog test is more severe than immersion in water.

Mr. Clamer.

MR. G. H. CLAMER.—It seems to me that in applying this salt-spray test there should be some distinction made between the test as applied to electro-positive coatings and as applied to electro-negative coatings. In applying the test to electro-positive metals, like zinc, the coating must necessarily be corroded or eaten away before rusting appears, but in applying the test to articles that are coated with electro-negative metals, like lead, the rusting appears at the pinholes which exist in the coating before the coating is destroyed, so that those articles which

are coated with electro-negative metal would probably show rusting almost immediately. I have found that in testing coatings of an electro-negative nature, immersion in pure water is about as quick a method as can be obtained. It develops pinholes quickly. The water should not be acidulated. The raw spigot water will show up the pinholes in a very few hours. The spray test recommended by Mr. Capp does not destroy the coating at all in the case of the lead-coated articles, but simply develops the pinholes, and the rusting then extends over the surface of the coating from those points. **Mr. Clamer.**

MR. CAPP.—In answer to Mr. Clamer I would say that if rust develops, the article has failed, because the only object of applying the protective coating is to prevent rust. If there are pinholes which allow the moisture to get through to the underlying metal which it is desired to protect, the protection has been inefficient. Therefore, any test which shows up the rust is useful. We use the same tests for all methods of protection, because the time factor is the only measure possible in the test. If there are pinholes, rust will develop at these pinholes, and the coating effectively has failed of its purpose, even though the coating in general has not been broken down in test. **Mr. Capp.**

PAINT PROTECTION FOR PORTLAND-CEMENT SURFACES.

BY H. A. GARDNER.

SUMMARY

Object.—The investigation had for its object the determination of what constitutes a suitable paint for the protection and decoration of exterior and interior stucco walls constructed of Portland cement, and for decorating and rendering wear-proof Portland cement floors.

Results.—The tests showed that the amount of free lime in fairly dry cement surfaces does not adversely affect high-grade oil paints. The tests also showed that zinc sulfate may be used with excellent results as a primer to neutralize free lime in damp cement surfaces which are to be painted. The scaling of varnish paints, which developed in several tests, apparently showed that resinous paints are not well suited for cement surfaces.

Conclusion.—Excellent results may be obtained by treating cement walls or floors with paints made with raw and heat-treated linseed and Chinese wood oil, containing durable, wear-resisting pigments.

PAINT PROTECTION FOR PORTLAND-CEMENT SURFACES.

BY H. A. GARDNER.

In many instances it has been found desirable to decorate certain kinds of Portland-cement structures with moisture-proof coatings.¹ The appearance is thereby made more pleasing and the cement is protected from the destructive action of the elements.

The author has occasionally come across statements which would tend to create an impression to the effect that Portland-cement surfaces are highly alkaline and cannot be satisfactorily decorated with oil paints. That such an impression is incorrect, seems to be the opinion of all investigators who have made painting tests of an extended nature. In fact, there is much evidence to show that Portland-cement surfaces are quite as well suited to receive paint coatings as iron, wood and other kinds of structural materials. Moreover, the results of well-conducted tests have shown that pure oil paints, such as are used upon wooden structures, may be used with equal satisfaction upon cement.

That the painting of cement is not a new art, is evident from the many references in the literature which describe methods of decorating and rendering damp-proof all classes of cement. Soon after Portland cement came into general use, it was customary to decorate certain kinds of constructions made from this material, the prevailing method being to apply varnishes containing copal, shellac, rosin or other resins, admixed with oil, turpentine and benzine. Subsequent to the application of such primers, a finishing coat of enamel made by grinding color pigments with the primer, was then applied. The principles underlying these old methods have since been followed with varying success, and up until a few years ago it was a common procedure for painters to apply a coat of varnish suction primer to cement and plaster walls, previous to painting them. Sur-

¹ This paper does not refer to treating cement masonry which is subjected to hydrostatic pressure and which may be in a very wet condition. Such masonry requires waterproofing rather than moisture-proofing treatment.

faces thus treated were filled, made less absorptive and from this standpoint in a somewhat better condition than bare walls, to receive the final coat of paint or enamel.

The suction varnishes or fillers made for this purpose were of two classes. One widely used was made of rosin dissolved in benzine and admixed with linseed oil. The rosin was sometimes made more suitable by dissolving therein certain drying agents such as red lead or lime. Lead or calcium resinate would thus be contained in these fillers. Varnishes made of the more expensive resins, such as Manila and copal, have also been used for the same purpose.

Whether these suction varnishes actually enter into chemical combination with the lime or other materials in cement or plaster, or whether they simply act as insulation coats, is a matter which has received considerable attention. If chemical reaction really occurs when a varnish coating is applied to cement, varnishes of the highest acidity, such as those made of colophony resin (common rosin), would be most efficient as lime neutralizers and better suited as primers, provided the durability of the coating is dependent upon the amount of calcium resinate which might be formed. Varnishes which have been prepared from the expensive resins generally show an acid number of from 5 to 10, while varnishes made from colophony resin (rosin) generally have an acidity of from 40 to 80. The author is inclined to doubt the possibility of lime neutralization by the use of suction varnishes. It is not a simple matter to produce calcium resinate under the most favorable conditions. Even molten rosin of 150 acidity requires prolonged heating at a high temperature, in order to take up as little as 2 to 3 per cent of lime. When a varnish of low or high acid value is applied in the cold to a cement surface, drying occurs in an exceptionally short time, and there is no opportunity for the small proportion of resin present in such an extremely thin coating, to effect a reaction which even at high temperatures takes place with the greatest difficulty. The effect which is really produced when a varnish is applied to a cement surface is not chemical, but of a physical nature, the varnish having a binding and filling effect upon the cement, similar to the binding and filling effect which is produced by the application of certain types of oils to cement surfaces.

Probably too much importance has been attached to the alleged destructive action of lime upon cement surfaces. It is well known that setting Portland cement develops free lime, but the amount to be found upon a Portland-cement structure should be considered as negligible in most instances, as far as it might affect a well-designed paint. If indeed it is advisable to neutralize this small amount of lime, previous to painting the cement surface, such a result is not to be properly accomplished with an organic substance but rather with an inorganic material which readily reacts therewith. A solution of zinc sulfate¹ (Macnichol's method) has proved most efficient for this purpose and has been used for many years with practical results, especially upon freshly laid cement. It might be well to point out at this place that the priming or ground-coating of cement is often improperly carried out, when clear primers of any kind are used. If, for instance, a clear varnish is applied to a cement surface, the primed surface is difficult to differentiate from the untreated area. Consequently, the workmen using clear primers often leave untreated laps or "holidays," as they are technically known. For this reason, the use of color primers should be adopted wherever possible.

The dusting of cement floors, which is brought about by abrasion, may be effectively stopped through the use of oil-pigment paints. If the floor has been freshly laid and is damp, the possibility of lime reaction may be removed by treating the surface with a solution of tinted zinc sulfate. The oil paint may then be applied. Boiled linseed oil, sometimes mixed with Chinese wood oil, may be used as the liquid portion of the paint. These oils have a remarkable binding action when applied to a cement surface. When mixed with pigment, they form paints which are eminently suited as first coaters for cement floors. The first coat will dry rapidly and form a dense surface. Over this may be applied a second coat and, if a high gloss surface is desired, a portion of varnish may be added to this final coat. The various floors of the author's laboratory were treated in this fashion, with prepared floor paints made from such materials, one week after the placement of the cement. The zinc-sulfate primer was used only upon the damp areas. The

¹ *Proceedings, Am. Soc. Test. Mats.*, Vol X, pp. 396-400 (1910).

floors have since been subjected to much abrasion from constant walking and the moving of heavy apparatus. Oils and chemicals of various kinds have come in contact with the floors, and soap and water have been used upon them very often for cleaning purposes. After three years' continued service, the floors have not dusted and have never required repainting. The paint films are still in good condition. This service record would tend to show that placed Portland cement may be made dust and wear-proof and highly desirable as a flooring material, through the application of oil paints.

In April, 1912, the writer instituted a series of tests to determine the durability of various types of paint upon Portland-cement surfaces exposed to the weather. The panels for the tests were prepared by constructing a long board wall to which was fastened expanded metal. A mixture of 1 part of Portland cement and 2 parts of clean Potomac River sand was made and applied to the expanded metal, forming a cement wall 3 in. in depth. The wall was divided into 35 sections or panels, each 30 in. wide and 40 in. high. Three coats of paint were applied to each panel by a practical journeyman painter. In order to make the test more severe, nearly all of the paints were applied in white. (Tinted paints are known to be much more durable than white paints.) A stripe of chrome green, 6 in. wide, was placed over the top of the third coat of paint, in order to determine whether the lime which might be present on the surface of the cement would have any effect upon the paint coating. Fading of the green to a yellow would indicate such action.

The general results of the tests at the end of a 2-year period, together with an outline of the composition of the paints tested, is given as follows:

Class No. 1.—Single-pigment paints made with white lead or zinc oxide ground in pure linseed oil.

These paints are in very good condition throughout (see Fig. 1).

Class No. 2.—Combination-pigment paints made of mixtures of white lead, zinc oxide or similar pigments ground in pure linseed oil.

These paints are in generally excellent condition (see Fig. 2).

Class No. 3.—Combination-pigment paints ground with mixtures of raw and heavy-bodied linseed oil or with treated Chinese wood oil. The viscosity of these oils requires the use

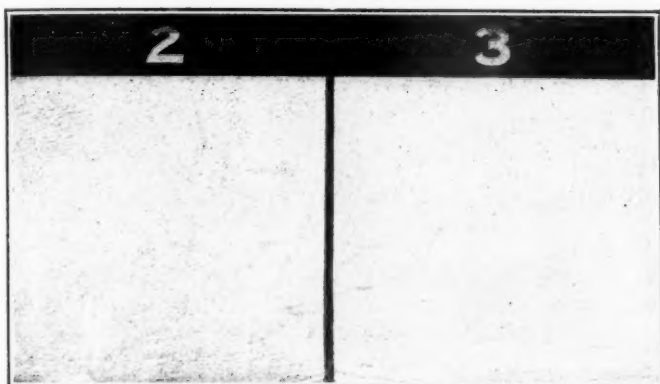


FIG. 1.—Single-Pigment Paints made with Pure Linseed Oil (Class No. 1). Very good condition.



FIG. 2.—Combination-Pigment Paints made with Pure Linseed Oil (Class No. 2). Excellent condition shown.

of considerable turpentine or other thinner in the manufacture of such paints, in order to make them of the right viscosity for application. Semi-flat surfaces are therefore produced during the drying. Such a finish is often preferred to a gloss surface.

Most of these paints are in excellent condition (see Figs. 3 and 4).

Class No. 4.—Single and combination pigments ground in oil varnishes containing acid resins.

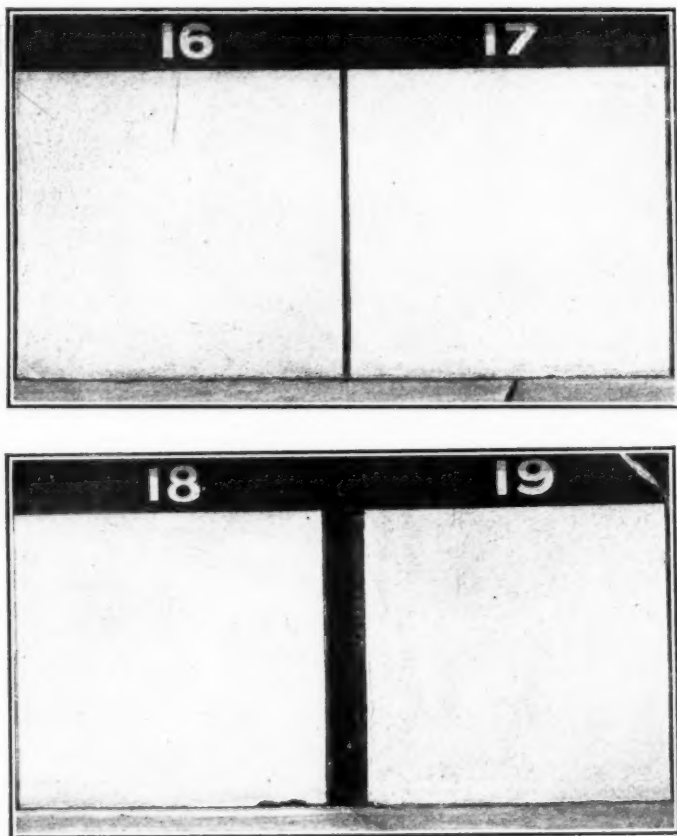


FIG. 3.—Combination-Pigment Paints made with Heat-Treated Linseed and Chinese Wood Oil (Class No. 3).
Excellent condition shown.

These paints are checking and scaling in many spots. Such varnish paints are apparently not suited to exterior exposure (see Fig. 5).

Class No. 5.—Paints containing resins dissolved in volatile spirits (spirit varnishes) with or without pigments.

These paints are not giving very satisfactory service, the clear varnishes having entirely decayed in some cases. Those to which pigment has been added are in somewhat better condition.

Class No. 6.—Paints made with single and combination



FIG. 4.—Combination-Pigment Paint made with Mixture of Raw and Heat-Treated Oil (Class No. 3). Excellent condition shown.

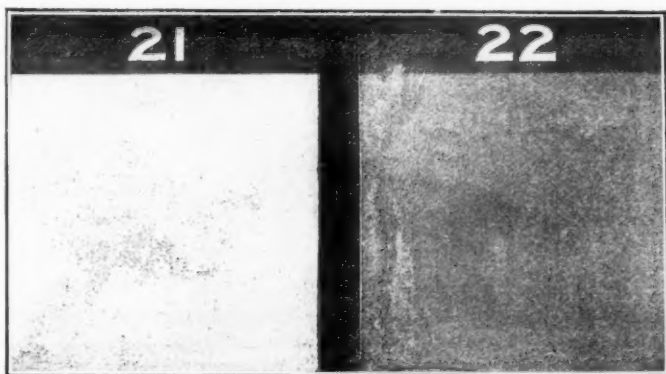


FIG. 5.—Varnish Paints Containing Acid Resins (Class No. 4).
Scaling and surface disintegration shown.

pigments ground in a water medium containing glue or casein as a binder.

These paints are chalking rapidly and are not moisture-proofing the cement. The pigment binder has been destroyed by the weather.

Opaque white pigments, such as basic-sulfate white lead, basic-carbonate white lead, zinc oxide, and lithopone, were present in the paints which gave the best results. In some of these paints there was also present a percentage of inert pigments, such as barytes, asbestine, whiting, china clay, gypsum and silica.

The results of these tests are quite in line with the results obtained by Ware and Schott¹ in a series of paint exposure



FIG. 6.—View of Various Prepared Oil Paints in White and Tints after Two Years' Exposure on Cement Construction. Most of these Paints are Class No. 3.

tests made upon exterior concrete surfaces. They also agree with previous long-time exposure tests made by the author.

As a result, therefore, it can be stated that concrete surfaces may be decorated with excellent results through the use of high-grade oil paints. When the cement surface is freshly laid and damp, such paints may be safely applied after treating the cement with a zinc-sulfate primer.

¹ "Paint Films as Protective Coatings for Concrete," *Journal of Industrial and Engineering Chemistry*, Vol VI, No. 3, p. 184 (March, 1914).

DISCUSSION.

MR. HERBERT ABRAHAM (*by letter*).—In view of the fact **Mr. Abraham.** that Mr. Gardner's conclusions appear to be based solely upon the behavior of specimens 22 in. square, and that actual field practice does not bear out his conclusions, I feel called upon to refute his statements.

In the opening paragraph of his paper, Mr. Gardner states as follows:

"The author has occasionally come across statements which would tend to create an impression to the effect that Portland-cement surfaces are highly alkaline and cannot be satisfactorily decorated with oil paints. That such an impression is incorrect, seems to be the opinion of all investigators who have made painting tests of an extended nature. In fact, there is much evidence to show that Portland-cement surfaces are quite as well suited to receive paint coatings as iron, wood, and other kinds of structural materials." . . .

This, however, is not borne out by a further statement of Mr. Gardner, in the paper referred to, wherein he states:

"A stripe of chrome green, 6 in. wide, was placed over the top of the third coat of paint, in order to determine whether the lime which might be present on the surface of the cement would have any effect upon the paint coating. Fading of the chrome green to a yellow would indicate such action." . . .

I am quoting this latter paragraph in view of the statement made by Mr. Gardner that there is no reaction in the cold between the lime and any material which might be applied over it. This is easily disproved by the simple experiment of spraying phenolphthalein solution on a concrete slab, and then applying any suitable organic acid, or acid resin, over it. It will be found that the red phenolphthalein color will be immediately discharged. Many authorities have proven without question that lime is set free when Portland cement hardens. Some have even calculated the percentage, which is substantial.

Mr. Abraham.

If the facts are as Mr. Gardner represents, why does he now recommend the zinc-sulfate treatment to neutralize the free lime? This method originally attracted some attention, but died a natural death, and deservedly so, some years ago. The zinc-sulfate treatment is not only impracticable, but valueless and costly. No one can tell beforehand how much zinc sulfate is needed to exactly neutralize the lime in cement. An excess of the zinc sulfate will harm the paint film more than the lime which it is intended to neutralize.

The writer has had considerable experience in manufacturing, marketing, and observing the behavior of paint and varnish coatings intended for, and used on, Portland-cement surfaces, and can state that coatings made from vegetable drying oils alone, in combination with pigments, are prone to failure. On the other hand, coatings containing acid resins or suitable organic acids, have been eminently successful in practice. The statement that a clear varnish when applied to cement surfaces cannot be differentiated from the untreated area, is manifestly incorrect. The writer has seen many square yards of cement surface treated with a clear filler containing an "acid resin." The area treated was considerably darker than the untreated area, and there was absolutely no possibility of leaving any place untreated without easily detecting it.

Mr. Gardner has given no formulas for the paints which he applied to the panels two years ago, other than in a very general way. Thus he states that paints of class No. 4 contained single and combination pigments ground in "oil varnishes containing acid resins," and because these checked and scaled in many spots, he concludes broadly that "such varnish paints are apparently not suited to exterior exposure." Let him give the exact formula and acid number in each case, and then we shall know what the facts are.

Mr. Dewar.

MR. JOHN DEWAR (*by letter*).—Mr. Gardner's paper should be of vital interest to architects, engineers and master painters, as it clears the way for the successful painting and decorating of cement and lime-plastered surfaces.

I am much pleased to learn of the value placed upon the use of the Macnichol zinc-sulfate treatment, to neutralize the lime in cement and plaster. I consider this process one of the very

important discoveries, made in recent years, for the benefit of **Mr. Dewar.** paintdom. In my operations as a painter, I have used this treatment ever since it was brought to the attention of the trade by Mr. Macnichol, its discoverer. It is now in general use by many of the leading master painters in this country and Canada, as well as abroad, who find it not only practical, but most satisfactory, eliminating all doubt in the mind of the painter as to just how his work will turn out. I have found it inexpensive, economical and easy of application. Mr. Gardner's suggestion to tint or lightly color the solution before applying it is a good one, as it enables the operator to cover the entire surface. I have used the Macnichol treatment with perfect success under conditions that, prior to my knowledge of it, I would have said no paint or color ever made would meet successfully. By following the directions set forth by Mr. Macnichol, in a paper read before the Society some years ago,¹ his treatment will ensure the results claimed for it. It does not affect the physical properties of the paint, and I would recommend its use.

MR. P. C. McILHINEY (*by letter*).—The question of the **Mr. McIlhiney.** painting of Portland-cement surfaces is a very important one, and is likely to increase rather than diminish in importance in the future.

The author gives the impression that the painting of Portland-cement surfaces presents no difficulty. If this is true, it should be hardly necessary to write an article upon the subject, because until some difficulty is encountered paints are generally applied to all kinds of surfaces; it is only when trouble appears that the subject of painting any particular kind of surface is brought forward. The reason generally given for the failure of ordinary paints when applied to Portland cement is that the free lime in the cement attacks the oil of the paint and prevents it from forming a durable connection with the surface. Incidentally the free lime affects certain pigments.

This trouble is a real one; it has been with us since Portland cement began to be largely used, and there are several methods which have been tried for overcoming the difficulty. The mere statement that ordinary oil paint made from linseed oil and the usual pigments may be successfully used upon fresh

¹ *Proceedings, Am. Soc. Test. Mats.*, Vol. X, p. 396 (1910).

Mr. McIlhiney. Portland cement is not convincing. Experience has shown that something more is required in the way of adaptation of the vehicle to the surface than is necessary when painting wood, for instance.

It is generally agreed by those having had experience in the matter that it is unsafe to paint fresh Portland cement with linseed oil simply combined with the pigment, and a glance at the issues of the paint and varnish journals will show that both the consumers and manufacturers of paints for concrete have found it necessary to do something further in the way of providing for the peculiar needs of concrete surfaces than would be suggested by the statement contained in this paper. As a matter of fact, in order to insure the adherence of paints to Portland-cement surfaces it has been found generally necessary to prevent the lime of the cement from acting upon the vehicle of the paint, and this is generally and successfully accomplished by the use of an acid primer or filler. The acid generally consists of some form of resin, this being the kind of acid which has been demonstrated to give the best results and to produce the hardest wear-resisting surface.

For out-of-door exposure, of course, the paints used must be of a nature to resist the weather as well as having the power to neutralize the free lime of the cement, but the series of tests made to determine the durability of paint upon Portland-cement surfaces exposed to the weather as described by the author, would seem to have little or no bearing upon the question of the need of these surfaces for special treatment in order to enable the paints to remain in place so as to have their durability tested. Probably the Portland-cement surface to which this paint was applied in the out-of-door test for durability was treated with some chemical solution before the paints were applied.

There may be differences of opinion as to the relative value of acid resins contained in the paint and of sulfate of zinc or some other chemical applied as a preliminary coating to the Portland-cement surface, as means for preparing the surface for painting or for effecting a combination between the applied paint coating and the Portland-cement surface; but there can be no doubt that some means must be adopted other than the mere painting of the concrete with an ordinary linseed-oil paint if the work is to be regularly successful.

Another part of the paper is devoted to a discussion of the **Mr. McIlhiney.** chemical reaction between acid resins and the free lime in Portland cement and the author's conclusion is that there is no such reaction. This conclusion is a remarkable one in view of the fact that a great variety of materials other than acid resin, many of which are much less acid than an acid resin would be, are well known to enter into decided reaction with the free lime of a Portland-cement surface; for example, sulfate of zinc and linseed oil, which are neutral substances, and all other kinds of acids including sulfuric, muriatic, oleic, etc., are universally admitted to enter into ready reaction with free lime. The successful practical results obtained by manufacturers of paints for concrete who use proper acid resins, would of themselves show strongly that an effective reaction takes place.

The author makes a plea for the addition of color to priming coats such as a coating of a resinous filler, on the ground that workmen are liable to leave untreated portions or "holidays" if the resinous solution is used clear. It is a little difficult for any one who has had experience in treating actual Portland-cement constructions to understand why this should be necessary, because there is no difficulty in seeing which portion of a floor, for instance, has been gone over with the filler and which is in its original untreated condition.

The suggestion which the author makes, briefly stated, is that manufacturers of concrete paints have made a mistake in adopting for their general practice the use of acid resins made into a solution with oils and thinners, and that the use of ordinary oil paints made from linseed oil is all that is required, with perhaps a preliminary treatment of the surface with sulfate of zinc. Perhaps the best reply that could be made to this is that the plans suggested by the author have been tried upon a large scale for years and have been found wanting.

MR. MAXIMILIAN TOCH (*by letter*).—**Mr. Gardner's paper** **Mr. Toch.** on new methods for painting cement with old and well-known materials, based on experiences of two years with cement or concrete slabs less than 3 ft. square, is very surprising, and the statement that he makes that linseed-oil paints are satisfactory does not seem to be borne out by his own experiments.

Mr. Toch. On paper the reaction between the lime in the concrete and a solution of zinc sulfate is quite ideal, but inasmuch as none of us know how much lime we are to neutralize we cannot use a quantitative proportion of zinc sulfate. If we do not use enough we shall have free lime, and if we use too much we shall have a surplus of crystallin zinc sulfate. At all events, an aqueous solution applied to concrete which is to be painted with an oil paint must be allowed to dry thoroughly or else the oil paint will not adhere. If the oil paint does adhere in the beginning it is pervious to water, and when the water gets through the film and dissolves out any surplus of zinc sulfate or any salts which may be present, it ruins the oil paint. This at least has been my experience with this mixture, and I believe that this has been the experience of others also, and that is why zinc-sulfate solution was dropped.

As far as painting a cement floor is concerned, it may be said that an oil paint of that kind is too soft on any floor. I have before me the analyses of the liquids used by Mr. Gardner on all of the 33 experimental slabs at Washington. Mr. Gardner concludes from these 33 experiments that linseed oil paints are the only ones suitable for this purpose; and he quotes the following: "Panels Nos. 2 and 3 made with pure linseed oil, and panels Nos. 16 to 19 made with linseed and China wood oils." No report is given of panel No. 1, which was painted with linseed oil, drier and turpentine. No comprehensive report is given of panel No. 12, which is 70 per cent soya bean oil and 30 per cent linseed oil. Panels Nos. 16, 17 and 18 are not coated with a material containing raw Chinese wood oils by any means, but heavy-bodied linseed oil with treated Chinese wood oil, of which the acid number is not given. Panel No. 19 contains 14 per cent of tungate drier, and would not come under his category. Panel No. 20 is not described, but contains 15 per cent of Kauri gum in japan in addition to Chinese wood oil and linseed oil. The acid number is not given.

Panel No. 21 is regarded as a failure, but contains 37 per cent of refined mineral oil, it being classified by Mr. Gardner as a varnish paint containing acid resin. The formula gives no indication of it whatever, it being a mineral oil—linseed oil—lime-water solution with 20 per cent of drier and 7 per cent of

turpentine; but the photograph shows this panel is about the same as panel No. 2. Panel No. 22, from the photograph, appears to be perfect, excepting in the upper left-hand corner where the concrete is cracked. When I saw this panel, No. 22, it looked as good as any of the others. Mr. Toch.

Panels Nos. 23 and 24 are omitted from Mr. Gardner's discussion. Panel No. 23 is an acid-resin paint, and panel No. 24 a Chinese wood oil paint.

The perspective photographs of panels Nos. 26 to 33, inclusive, are not distinctive enough to be commented upon, but panels Nos. 26, 27, 28, 29 and 30 from the photograph appear to be perfect, and yet they are all of different types. Panel No. 29 is a glue solution, and shows up in the photograph as good, if not better, than some of Mr. Gardner's oil-paint specimens.

I have examined upwards of thirty paints that are on the market for the purpose of painting Portland-cement construction, and I have not found a single one of them which is a raw linseed-oil paint. Every one of them when poured on concrete which is stained red with phenolphthalein discharges the color of the phenolphthalein after a few moments. It strikes me that Mr. Gardner's conclusions are premature, and that it would be well for him to carry his experiments further.

I know of a number of concrete buildings exposed to the elements which have been painted with the acid-resin type of paint with perfect success, and which have stood for more than two years. I also know of a large number of buildings in which the floors and walls have been treated with the acid-resin type of paint, and have given perfect satisfaction, including factory floors and power-house floors where a great deal of heavy traction is carried on continually.

I would refer Mr. Gardner to a book on "Paint Making and Color Grinding," by Charles L. Uebele (page 331), published in 1913, in which the reasons are given for the abandonment of the zinc-sulfate solution as a primer for Portland-cement concrete.

I contend that Mr. Gardner's report is incomplete, and that no correct conclusions can be drawn from it.

MR. E. E. WARE (*by letter*).—The results of the tests on paint protection for concrete surfaces, as reported by the author, Mr. Ware.

Mr. Ware. agree very well with results of tests carried out at the University of Michigan, reference to which has been made.

It seems that altogether too much stress has been laid on the effect of lime in concrete on paint coatings. While it is true that lime solution will saponify linoloxyn even in the cold, it is evident that under the conditions existing on a concrete surface, sealed by an impervious film of paint, lime has very little opportunity of coming into sufficiently intimate contact with the linoloxyn film to exert its disintegrating effect.

It is to be hoped that Mr. Gardner will continue his tests through a period of repainting, for it seems quite certain that coaters possessing the elastic and weatherproof qualities of a properly compounded linseed-oil paint, will demonstrate their superiority even more conclusively than they have done in the tests already reported.

Mr. Gardner. MR. H. A. GARDNER (*Author's closure, by letter*).—In answering Mr. Abraham's remarks regarding the chrome-green paint as an indicator of lime, I may say that my article contains no statement as to any reported results on this test.

I am, of course, aware that lime is developed in the setting of Portland cement, but my contention is that such lime is not reactive to linseed oil or similar organic substances upon a dry cement surface. To prove this is a simple matter. If phenolphthalein is dissolved in alcohol and placed upon the surface of a dry Portland-cement construction, no color will be developed. If the same solution be placed upon a moist or wet Portland-cement construction, a pink color will be developed immediately, showing inter-action with the alkaline lime present upon the surface of the cement. This very delicate test for lime shows that it reacts only in the presence of moisture. When Portland cement which is damp or wet is to be painted, the writer has always advised the use of the zinc-sulfate priming treatment, in order to neutralize any lime which may be present on the surface of the wet cement. This Macnichol zinc-sulfate treatment has proved in actual practice for many years to be extremely satisfactory and practical. It is, moreover, a treatment which is not covered by patent and can be used by any one. The cost of a gallon of zinc-sulfate solution for priming purposes would be in the neighborhood of twenty cents. The painter

who wishes to use such solution can make it himself very readily and without trouble. The drying of the zinc-sulfate solution upon a Portland-cement surface is very rapid, and linseed oil paints can be applied the next day with excellent results. Mr. Gardner.

Detailed formulas were not given in my paper, as Mr. Abraham states. This was due to the fact that the paints were classified into several groups and the writer desired at the time to report only upon the groups. The formulas of these paints, however, are available and will be published in detail at the next inspection.

In reply to Mr. McIlhiney, I wish to state that the cement panels were not treated with any solution previous to applying the paints, except in the instances referred to, where four of the panels were brushed with a zinc-sulfate solution. The paints were applied to the other panels without previous treatment.

That the method of treating cement, which I have suggested, has not been found wanting in practice may be confirmed by consulting the records of work of the most prominent master painters of the United States. For many years they have been using the zinc-sulfate treatment, followed by oil paints, and with marked success. Testimony to this effect is to be found in the Proceedings of the International Association of Master House Painters and Decorators of the United States and Canada.

Answering Mr. Toch, I might state that my experiments in painting cement have been carried on for several years, and the tests cited in my latest paper confirm the results obtained in many of the earlier tests. The statement made by Mr. Toch, regarding the abandonment of the zinc-sulfate treatment, is incorrect. This treatment is still being used by the most prominent master painters of the United States in painting large cement structures. Moreover, the general usefulness of this method is well known, as the following item from the *Washington Evening Star* of September 26, 1914, will show:

"To the Editor of 'The Star':

"In your issue of July 18 you published a letter from W. F. Andrews, secretary Master House Painters and Decorators' Association of Washington, D. C., giving credit to Mr. Charles Macnichol of this city for first having

Mr. Gardner. successfully put into practice the preliminary treatment with a solution of zinc sulfate of concrete surfaces to be painted. It gives me pleasure to corroborate Mr. Andrews' statement, and also to comment favorably on the magnanimity of Mr. Macnichol in giving this formula gratuitously to the public. Having been present at the meeting of the American Society for Testing Materials at which the method was given publicity, I well recall its favorable reception by manufacturers and consumers of protective coatings, many of whom had almost despaired of finding an economical method of painting concrete surfaces.

"The Geological Survey, in publishing the results of investigations of both paint materials and concrete, was very glad to assist in disseminating information concerning this promising method of treatment, and in the chapters from the Annual Volume of Mineral Resources, 1909, on 'Mineral Paints' and the 'Cement Industry,' outlines were given of the Macnichol method, with due credit to the genial citizen of Washington who suggested it. It should be a source of satisfaction to Mr. Macnichol to know that this method, which now is officially known as the 'Macnichol method,' has proved universally so satisfactory. The public is greatly indebted to Mr. Macnichol, and he is entitled to all the honor that can be accorded him for his disinterested service.

"E. F. BURCHARD,

"Geologist, United States Geological Survey."

Answering Mr. Toch's statement regarding the composition of some of the paints, I might say that panel No. 1, referred to by Mr. Toch, was made with raw linseed oil. It gave good results. The oil paint applied to panel No. 12 was made up with soya bean oil and heavy blown linseed oil. This also gave good results. The paints applied to panels Nos. 16, 17, 18 and 19 were also oil paints, the Chinese wood oil being heat treated to body it up to proper consistency. Panel No. 20, referred to, contained, as Mr. Toch states, 15 per cent of Kauri gum japan. Its surface is deeply checked. Panel No. 21 contains 20 per cent of siccative (resin drier).

Mr. Toch cites the phenolphthalein test as one that he applied to determine whether commercial paints for cement surfaces, as now found upon the market, are of the oil type. Because the liquid in such paints discharged the red color which phenolphthalein produces upon cement surfaces, he assumes that such paints are not of the oil type. This method of paint analysis is at least simple, but unfortunately is not of a determinative character, since ordinary raw linseed oil, as well as

varnish and resins in solution, discharge this pink color with Mr. Gardner. almost equal rapidity.

The article ascribed to Mr. Uebele refers to the zinc-sulfate treatment and indicates why it is used. In previous and subsequent articles by Mr. Uebele, the zinc-sulfate treatment is advised for the priming of cement previous to the application of paints (see *The Painters' Magazine*, 1910 to 1914).

METHODS FOR DETERMINING THE MELTING POINTS OF ASPHALTS.

BY J. G. MILLER AND P. P. SHARPLES.

SUMMARY.

Melting point determinations were made on a variety of asphalts by the cube method in oil, cube method in air (Special Committee, American Society of Civil Engineers), cube method in air (Barrett), and by the ring-and-ball method as practiced under five different specifications.

Conclusions.—The cube method in oil has not proved satisfactory in the determination of asphalt melting points.

The cube method in air, as specified by the Special Committee of the American Society of Civil Engineers, has proved very satisfactory.

The ring-and-ball method would prove an acceptable and easy laboratory method, but no satisfactory concordant results can be obtained unless the method is thoroughly standardized.

METHODS FOR DETERMINING THE MELTING POINTS OF ASPHALTS.

BY J. G. MILLER AND P. P. SHARPLES.

The methods for the determination of the melting points of coal-tar pitches have been practically standard for a number of years. What is known as the "half-inch cube method in water"¹ is the recognized standard for the determination of the melting point of pitches of a melting point below 170° F. The attempt has been made to adapt the method to higher melting

TABLE I.—COMPARISON OF HALF-INCH CUBE METHODS.

Method.	Bath.	Apparatus	Size of Beaker, cc.	Temperature at Start, deg. Fahr.
Oil-bath, Barrett Mfg. Co.	Cottonseed Oil	One beaker	600	60
Sp. Com. Am. Soc. C. E. ²	Air	Two beakers	Air, 300 Cottonseed oil, 800	77
Air-bath, Barrett Mfg. Co.	Air	Special Copper Oven	Normal.

NOTE.—In all methods the specimens are $\frac{1}{2}$ -in. cubes, and are suspended at a height of 1 in. from the bottom of the apparatus; the suspending wire is of copper, No. 12 B. & S. gage; the rate of heating is 9° F. per minute; and the melting point is taken when cube touches the bottom of the apparatus.

points by the substitution of a cotton-seed oil bath for the water bath, but the results have not been wholly satisfactory. The same methods applied to asphalts have not proved satisfactory owing to the low range of temperatures within which the method is adaptable; also because some asphalts have a specific gravity below that of the liquid bath.

The Special Committee (on Road Materials) of the American Society of Civil Engineers² has proposed a method

¹ S. R. Church, "Methods for Testing Coal Tar and Refined Tars, Oils and Pitches Derived Therefrom," *Journal of Industrial and Engineering Chemistry*, Vol. 3, No. 4, p. 227 (April, 1911).

² See blank forms (and accompanying instructions) printed and distributed by Special Committee on Road Materials of the American Society of Civil Engineers (published in "Good Roads" (N. Y.), for June, 1909, Vol. X, No. 6, p. 208). Also described by Prévost Hubbard, "Dust Preventives and Road Binders," p. 351, John Wiley and Sons, 1910.

TABLE II.—COMPARISON OF RING-AND-BALL METHODS.

Method.	Size of Ring (brass).			Size of Attached Wire, in.	Diameter of Ball (steel), in.	Bath.	Size of Beaker.	Amount of Liquid.	Distance of Ring from Bottom, in.	Rate of Heating, per minute.
	Diameter, in.	Depth, in.	Thickness, in.							
Barrett Mfg. Co., Chicago Laboratory . . .	$\frac{11}{16}$	$\frac{5}{8}$	$\frac{1}{64}$	$\frac{5}{32}$	$\frac{3}{8}$	Glycerin for high M. P. water for low M. P.	600 cc.	400 cc.	1	Water—5° C. Glycerin—7° C.
Barrett Mfg. Co., Philadelphia Laboratory . . .	$\frac{5}{8}$	$\frac{3}{8}$	$\frac{1}{8}$	$\frac{1}{4}$
Standard Oil Co. and Atlantic Refining Co. . .	$\frac{5}{8}$	$\frac{1}{4}$	$\frac{3}{32}$	$\frac{1}{16}$	$\frac{3}{8}$	Water or Oil.	3 in. diam.	2 in.	1	10° F.
Gulf Refining Co.	$\frac{5}{8}$	$\frac{3}{8}$	$\frac{3}{32}$	$\frac{1}{4}$	Glycerin	200 cc.	Two-thirds full	$1\frac{1}{2}$	5° F.
U. S. Asphalt Refining Co.	$\frac{5}{8}$	$\frac{1}{4}$	$\frac{3}{32}$	Adjustable support	$\frac{3}{8}$

NOTE.—The melting point in all cases is taken when ball touches bottom of apparatus; the temperature at the start is not given. At the Chicago laboratory of the Barrett Mfg. Co. the liquid is stirred.

TABLE III.—COMPARISON OF MELTING POINTS OF HARD, MEDIUM AND SOFT ASPHALTIC MATERIALS.
ALL RESULTS IN DEGREES FAHRENHEIT. RATE OF TEMPERATURE RISE IN ALL CASES=9° F OR 5° C. PER MINUTE.

Material.	No. 1, 4-in. Cube, Cotton- Seed-Oil Method.	No. 2, Sp.Com., Am. Soc. Mfg. Co., C. E. Air-Bath Method.	No. 3, Barrett Mfg. Co., Air-Bath Method.	Difference between		No. 4, Chicago Oil Co. & Barrett Mfg. Co., Ring-and Ball ¹ Method.	No. 5, Standard Oil Co. & Atlantic Ring-and Ball ¹ Method.	Difference between			No. 6, Chicago Barrett Mfg. Co. and Ring-and Ball ² Method.	No. 7, Standard Oil Co., Ring-and Ball ² Method.	No. 8, 4-in. Cube in Water.	Differ- ence between Nos. 7 and 8.
				Nos. 1 and 2.	Nos. 2 and 3.			Nos. 4 and 5.	Nos. 4 and 2.	Nos. 5 and 6.				
Atlantic Rfg. Co., Philadelphia Asphalt	292	277	262	15	15	280	280	20	3	17
Atlantic Rfg. Co., Asphalt.....	293	250	236	13	14	255	235	20	5	15
Paraffine from Standard Oil Co.....	...	250	239	...	11	...	235	25
Hydrolene, Sun Co.....	...	194	187	...	7	...	175	19
Texas Asphalt.....	246	230	214	16	16	237	212	25	7	18
Oxidized Flux Oil.....	...	160	155	...	5	161	161	20	21	1	178	160	178	18
"D" Grade Asphalt.....	...	144	140	...	4	158	139	19	14	5	146	131	...	15
Chicago Mixed Asphalt.....	...	124	118	...	6	133	117	16	9	7	128	113	...	15
Flux Oil.....	...	115	107	...	8	112	105	7	-3	10	109	97	...	12
Flux Oil.....	...	108	101	...	7	107	100	7	-1	8	105	94	...	11

¹ Glycerin bath used.

² Water bath used.

which removes the difficulties encountered, both for the higher melting-point, coal-tar pitches and for the asphalts. The method, however, has not been widely adopted, and the asphalt manufacturers quote melting points by the ring-and-ball method.

In the laboratories of the Barrett Manufacturing Co. a method¹ similar in principle to the method proposed by the Special Committee of the American Society of Civil Engineers has been adopted. This also obviates the difficulties encountered in determining the melting point of hard coal-tar pitches and asphalts by the "half-inch cube" method.

TABLE IV.—EFFECT OF USING A HEAVY WIRE IN THE HALF-INCH CUBE METHOD OF TEST FOR ASPHALTS.

Material and Method.	Melting Point, deg. Fahr.		
	B. & S., No. 12, 0.0808 in. diam.	No. 12, Stubbs, 0.109 in. diam.	Difference.
Atlantic Asphalt, Sp. Com., Am. Soc. C. E. Air-Bath Method.....	250	254	4
Soft Asphalt, Sp. Com., Am. Soc. C. E. Air- Bath Method.....	134	135	1
Medium Asphalt, Barrett Mfg. Co. Air- Bath Method.....	196	197	1
Hydrolene, Barrett Mfg. Co. Air-Bath Method.....	187	191	4

Tables I and II show a comparison of apparatus and methods used in the half-inch cube methods and the ring-and-ball methods.

The great discrepancies observed between the melting points as determined by the ring-and-ball method in the above-mentioned laboratories, and the melting points given by the companies furnishing the asphalt, led to an investigation of the method. It was found that the method as employed in different laboratories differed essentially in details, especially in the dimensions of the apparatus and the rate of heating. As the rate of heating is known to give wide variations in results, the different methods were made comparable by adopting the widely

¹ S. R. Church, "Methods for Testing Coal Tar and Refined Tars, Oils and Pitches Derived Therefrom," *Journal of Industrial and Engineering Chemistry*, Vol. 5, No. 3, p. 195 (March, 1913).

used rate of 5°C . per minute. The results with different asphalts by the different methods are given in Table III.

The necessity for standardizing the ring-and-ball method is very clearly shown by the comparison of the results from the apparatus of the Chicago laboratory of the Barrett Manufacturing Co. and that of the Standard Oil Co. The very surprising difference of 25°F . was brought out, which is as much as the difference between the Special Committee of the American Society of Civil Engineers' method and the ring-and-ball method of the Standard Oil Co.

It is also to be noted that the cube methods cannot be compared with the ring-and-ball method even by the introduction of a factor, since the relative difference depends on the kind of asphalt.

In Table IV is shown the effect of using a heavy wire in the half-inch cube method.

CONCLUSIONS.

The experience with the methods in the laboratory seems to indicate:

1. That the oil-bath method is inapplicable to asphalts, owing to their solubility in the oil;
2. That the Special Committee of the American Society of Civil Engineers' modification of the cube method gives concordant results and is a dependable method for check results;
3. That the ring-and-ball method has certain advantages for ease of laboratory manipulation, especially for low melting-point asphalts;
4. That if the results by the ring-and-ball method are to be made comparable, every detail of the apparatus and method of testing must be specified.

SPECIFICATIONS AND TESTS OF GLUE.

BY OSCAR LINDER AND E. C. FROST.

SUMMARY.

In this paper the authors give the results of tests of cabinet glue. They describe the commercial grades of glue and the method used for grading. The value of various tests and their relation to the strength of glued joints are discussed. The requirements of specifications for this material are outlined and the necessity for suitable treatment is shown.

SPECIFICATIONS AND TESTS OF GLUE.

BY OSCAR LINDER AND E. C. FROST.

The authors wish to bring to the attention of the Society a series of tests which were made for the purpose of collecting data for a specification for cabinet glue. The information in the literature on this subject is meager and somewhat conflicting, and does not yield a sufficient amount of reliable data for establishing the requirements of such a specification.

Glue is made principally from animal hides and bones; the better qualities from hides, the poorer ones from bones, although most glue found on the market contains materials from both hides and bones. The glue is furnished in flakes or sticks, or in ground form as granules or powder. It is graded by the manufacturers mainly by its "jelly strength," that is, by the rigidity or strength of a jelly made in a certain standard manner. This strength is generally not determined in absolute figures but by comparison with an arbitrary set of standards (Cooper's Standards being most commonly used) in accordance with which the quality is designated to be from "2" (worst grade) to "AA-EX" (best grade). The different grades are as follows: 2, $1\frac{7}{8}$, $1\frac{3}{4}$, $1\frac{5}{8}$, $1\frac{1}{2}$, $1\frac{3}{8}$, $1\frac{1}{4}$, 1, 1-X, 1-EX, A-EX, AA-EX. Some suppliers use designations of their own for some of the grades, a practice which is rather confusing to the consumer.

In general this grading is satisfactory and it has been found through experience that the grades showing the greatest jelly strength are the best grades for general use. The grading has, however, the obvious disadvantage of being entirely arbitrary, and the results obtained depend absolutely on the standards used. These standards are not always accessible to the consumer and there exists some doubt as to the agreement of standards used by different laboratories.

The tests which are outlined in this paper were selected from published articles by various authors and were supplemented by direct strength tests of glued joints. This gives a good check on the value of the various tests.

In Table I are listed the results of tests of a series of 25 samples of glue submitted by twelve of the most prominent glue manufacturers and dealers in this country, letters being substituted for the suppliers' names.

The breaking strength reported is in each case an average of six tests, and is in some cases erratic, due to the fact that sometimes the glue was stronger than the wood. The breaking strength tests were made as follows: For each test, three maple blocks were used, one piece 2 by 1 by 1 in. and two pieces 2 by 1 by $\frac{1}{2}$ in. These blocks were heated to 50° C. and glued together, so as to have 4 sq. in. of glued surface with the pieces flush at one end, the large block in the middle. The glue was prepared by soaking in cold water for 12 hours and then heating to 65° C. (149° F.) for 1 hour, whereupon it was applied to the test specimens at this temperature. After gluing, the test specimens were immediately put under a pressure of 100 lb. and kept there for 12 hours. They were then removed and at the end of another 12 hours the middle block was pushed from between the other two blocks on a universal testing machine and the breaking strength noted.

Some comments on the value of the tests given in Table I and of additional tests are given below.

Appearance.—The appearance is of no great help in judging the quality of glue as long as it is clean and free from excessive bubbles. Low quality glues are frequently light and clear and of fine appearance. An excessive number of bubbles indicates decomposition during manufacture.

Behavior on Soaking.—The tests show that a good glue should absorb much water, the flakes remaining firm and keeping nearly their original shape. The actual amount of water which a glue is capable of absorbing in a given time would be a good requirement to include in a specification, if it were possible to determine it accurately. This determination has, however, been found to be unreliable because its results are affected greatly by the thickness and size of the glue flakes or particles, as well as by the position of the particles while being tested. A high water absorption of glue indicates, in general, a high jelly strength, although there are exceptions, for example, sample No. 22.

TABLE I.—TESTS OF GLUE.

Sample No.	Appearance.	Behavior on soaking 30 g. glue in 80 g. water for 12 hours.		Moisture, per cent.	Ash.		Condition of Jelly on Cooling, 20-per-cent Solution.	Viscosity at 30° C., 15-per-cent Solution (Scott), seconds.	Average Breaking Strength, lb.			Relative Price, per cent.
		Absorption.	Condition.		Per cent.	Appearance.			1 part dry glue, 3 parts water.	1 part dry glue, 4 parts water.	1 part dry glue, 5 parts water.	
A { 1 2 3	Flake, mediumly dark Flake, light Sticks light	Good Fair Good	Firm Slightly slimy Firm	13.8 14.0 13.6	5.27 3.61 4.25	White White White	Firm Slightly fluid Firm	29.0 22.0 22.0	6360 6218 7715	5337 3710 5082	81 75 84
B { 4 5	Flake, light Ground, light	Good Good	Very firm Firm	13.4 14.2	2.31 3.27	Light brown Light brown	Firm Firm	28.0 32.0	7350 7260	5251 5821	4440	81 94
C { 6 7	Flake, light Flake, light	Good Good	Firm Firm	15.0 13.0	4.72 4.50	White White	Firm Slightly fluid	23.5 21.0	4860 5259	4365	94 87
D { 8 9	Flake, clear, light Ground, light	Good Good	Very firm Very firm	13.0 13.5	2.60 3.84	White White	Firm Firm	48.0 70.0	7720 7740	7410 7400	4785 6448	80 97
E { 10 11	Flake, mediumly dark Flake, clear, light	Fair Good	Slightly slimy Firm	12.5 12.9	2.50 5.47	White Dark, fused	Firm Firm	32.0 24.5	5604 5050	7110	4070	75 69
F { 12 13	Flake, clear, light Ground, light	Fair Poor	Soft Slimy	13.5 13.1	3.20 2.08	Dark, fused Fused slightly	Firm Fluid	25.5 18.0	6553 5716	4296	81 94
G { 14 15 16 17	Flake, mediumly dark Ground, mediumly dark Ground, light Thick flakes, light.	Poor Fair Fair Fair	Slimy Soft Soft Slimy	15.2 14.2 15.6 13.3	2.32 3.14 3.74 2.50	Reddish White Brown, fused slightly Brown, fused	Slightly fluid Slightly fluid Slightly fluid Fluid	19.0 20.0 20.0 18.0	5553 4885 4414 5896 2254	78 84 87 81
H { 18 19	Ground, light Ground, light	Good Good	Mediumly soft Mediumly soft	13.1 14.0	2.60 2.65	Fused slightly Dark, fused	Firm Firm	25.5 28.0	6762 6415	4746 6072 4296	94 87
I { 20 21	Flakes, clear, light Flakes, mediumly dark	Fair Good	Soft Medium	16.3 13.5	2.27 2.77	White Light, fused slightly	Fluid Fluid	16.5 21.0	7745 7218	3566 5076	81 75
J 22	Flakes, clear	Good	Firm	12.2	3.83	Light	Fluid	20.0	5990	3384	84
K { 23 24	Flakes, clear, light Ground, mediumly dark	Good Good	Firm Medium	12.0 14.2	2.53 4.79	Light Dark, fused	Slightly fluid Slightly fluid	25.5 21.0	6370 4493	5018	81 100
L 25	Flakes, light, trans.	Good	Firm	12.6	1.60	Dark, fused	Firm	25.5	6779	4358	97

Reaction of ash, alkaline in all samples.

Chlorine and phosphoric acid present in the ash of every sample.

No volatile acids in any sample.

The price of the different samples is given in relative figures based on the price of the highest sample, No. 24.

Moisture.—The moisture determination helps little in judging the quality of a glue as long as it is not extreme either way. In about 40 glue tests which the authors made (outside of the tests listed in Table I) the moisture was never below 10 nor above 17 per cent and generally between 11 and 15 per cent (average 13.5 per cent). The moisture is best determined by breaking about 2 g. of the glue into small pieces and noting the loss in weight upon heating at 115° C. (239° F.) to constant weight (about 1½ hours). The authors at first prepared the samples by rasping them by means of a coarse file, but found the rasping caused a considerable amount of the moisture to evaporate.

Ash.—About 6 per cent of ash is permissible in glue; the actual amount is generally found to be between 2.3 and 4.5 per cent (average 3.3 per cent). The nature of the ash indicates the kind of stock used and the treatment of the stock during the manufacture of the glue.¹ Evidently the glues listed in Table I were all made from a mixture of bone and hide stock.

Reaction.—The reaction of the samples listed in Table I was not determined; all glue should, however, be tested in jelly form with litmus paper and its reaction should be neutral, or at least very nearly so. Occasionally samples are found to be slightly acid or alkaline, which results when the neutralization of the acid used in the process of manufacture has been carried on insufficiently or to excess. The reaction is best determined in a 10-per-cent glue jelly; for example, the same jelly which is used for the foaming test. The jelly must of course be freshly prepared.

Jelly Strength.—At the time when the tests listed in Table I were made, the authors had no standards for the jelly strength and had to confine themselves to a comparison of the 25 samples under test. The manner in which the jelly-strength test is conducted is as follows: The glue is soaked in cold water for about 16 hours, then melted at 160° F. on the water bath. The proportions of glue to water are generally such that the resulting jelly will contain from 7 to 10 per cent of the glue according to its quality (7 per cent for the better grades and 10 to 15 per cent for the lower ones). The jelly is then cooled to about 45° F. in

¹ See Lunge, "Chemische Technische Untersuchungsmethoden."

an ice-box and its firmness (strength or consistency) judged by pressing with the fingers, and compared to that of the jellies prepared from a set of standard samples made at the same time and treated in the same manner as the sample under test.

The jelly strength is one of the most characteristic tests for glue, but its accuracy depends, unfortunately, on a set of standards which are not accessible to everybody and the accuracy

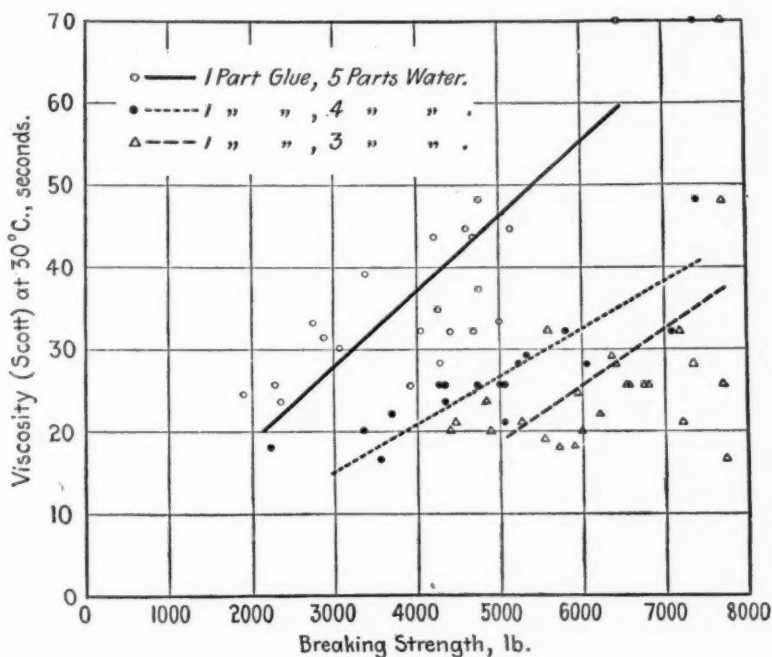


FIG. 1.—Breaking Strength of Glued Joints.

of which is hard to control. The authors have tried out several instruments constructed along the lines of the well-known Scott glue tester, and intended to do away with standard samples, but have always been able to determine the jelly strength with greater accuracy and less trouble by the old-fashioned "finger test." The chief disadvantage of the mechanical glue testers is their failure to register resiliency.

Viscosity.—The viscosity of the melted glue solution seems

to be the best single test that may be applied to a glue; common low-grade bone glues invariably show low viscosity. The relation between the viscosity and breaking strength of glue is unmistakable, and is plotted for convenience in Fig. 1. In addition to the strength tests given in Table I, Fig. 1 contains a series of other tests made under the same conditions and listed in Table III. The viscosity test should not be taken as final, however, because hide glues of low jelly strength may show high viscosity. It is, therefore, best to determine and specify both the jelly strength and the viscosity. In general, however, the jelly strength is approximately proportional to the viscosity.

TABLE II.—RESULTS OF TESTS OF COOPER'S STANDARD GLUES.

Grade No.	Viscosity at 30° C. (Engler).	
	Specific.	Time, seconds.
A-EX.....	6.9	358
1-EX.....	9.8	511
1-X.....	5.2	270
1.....	8.3	435
1 $\frac{1}{4}$	6.4	330
1 $\frac{3}{8}$	4.5	235
1 $\frac{1}{2}$	4.3	225
1 $\frac{5}{8}$	4.2	216
1 $\frac{3}{4}$	2.6	135
1 $\frac{7}{8}$	2.3	122

This is shown in Table II, which gives the results of tests of a set of Cooper's standard glues.

Grade No. 1 was evidently mislabeled by the supplier, as it showed a jelly strength equal to the A-EX grade. For convenience, the figures given in Table II are plotted in Fig. 2.

In laboratories where large numbers of glue samples are tested, it is customary to determine the viscosity on the same sample as used for the jelly-strength test immediately after melting and before cooling. Scott's viscosimeter is used generally and the viscosity is determined at 150° F. The authors found it preferable to determine the viscosity on a specially prepared fresh

jelly of a fixed strength, containing 15 per cent of glue (figured as anhydrous glue) in distilled water. The manner of preparing the jelly must be carefully considered because it is liable to affect the results. The procedure adopted by the authors is to soak the glue in the required amount of water at room temperature for from 12 to 16 hours, then to melt it on a water bath at a tempera-

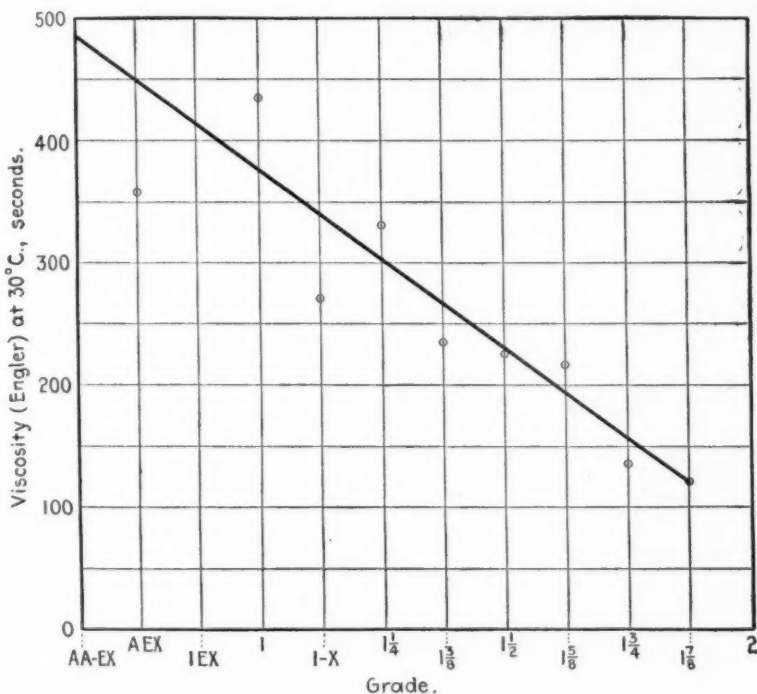


FIG. 2.—Viscosity (Engler) of Different Grades of Cabinet Glue (30° C., 15-per-cent jelly).

ture of not more than 66° C. (150° F.) for 1 hour under occasional agitation and then to cool and test its viscosity immediately at 30° C. (86° F.). The temperature of 30° C. was adopted because it is easily maintained.¹ Since making the tests tabulated in Table I, the authors have adopted the Engler viscosimeter in their work in place of the Scott. A conversion curve

¹ See Lunge, "Chemische Technische Untersuchungsmethoden."

of Engler to Scott viscosity for a 15-per-cent glue jelly at 30° C. (86° F.) is given in Fig. 3.

Breaking Strength.—In making the tests tabulated in Table I, all samples were first tested using 1 part of glue (by weight) to 3 parts of water. The glues showing the best results were selected for the next series of tests, using 1 part of glue to 4 parts of water; likewise the five best glues of this test were selected for the last series, using 1 part of glue to 5 parts of

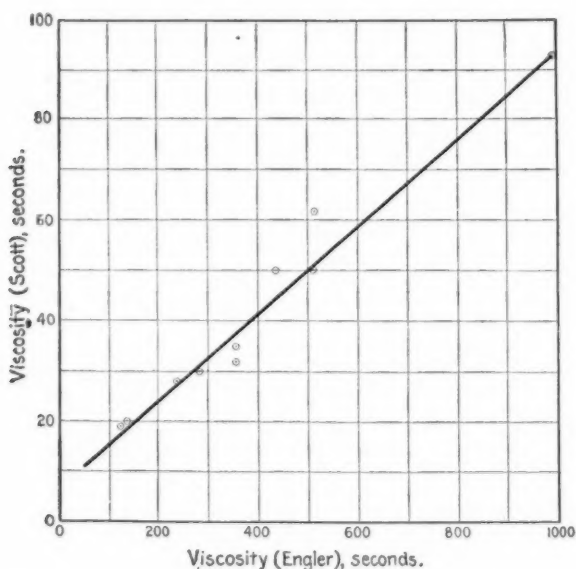


FIG. 3.—Viscosity of Cabinet Glue (30° C., 15-per-cent jelly) by Engler and Scott Viscosimeters.

water. The relation between breaking strength and viscosity, and therefore also jelly strength, has been mentioned above and is shown in Fig. 1.

Odor.—Some grades of glue have an offensive odor of decomposition, showing them to be poorly preserved or to have originated from unsuitable glue stock. This odor is not generally noticeable in the solid glue but appears upon melting of the glue jelly. While such a glue may show good initial adhesive strength,

its use should not be permissible except for purposes and in places where its odor is not offensive.

Grease.—The presence of grease in the glue is highly undesirable because it lowers the binding properties of the glue. In melting the glue jelly, any grease which may be present will also melt and float on the surface, where it can be detected. The fat may be determined quantitatively by heating the glue solution with dilute hydrochloric acid, cooling, and extracting the fat with ether. Absence of grease should always be required.

Foaming.—In many uses of glue, especially in gluing machines, excessive foaming is highly undesirable. A simple test is to beat about 300 cc. of a freshly prepared jelly containing 10 per cent of the original glue for 1 minute in a cylindrical beaker about $2\frac{1}{2}$ in. in diameter, at a temperature of 150° F. A good quality of glue forms a layer of foam not more than $\frac{1}{4}$ in. thick in this test. The test is naturally crude, but answers the purpose and is frequently made.

Preservatives.—The addition of preservatives is not objectionable as long as they are not of such character or added in such quantity as to render the glue poisonous or to affect its qualities.

The following tests are recommended by the authors as most important: Viscosity, Jelly Strength, Odor, Reaction, Grease, Foaming, Ash, Moisture, Appearance.

By means of the above tests, the quality of cabinet glue can be readily specified and checked. For example, for a good grade of cabinet glue, requirements for a jelly strength of grade 1-X and a viscosity of 350 seconds (by Engler viscosimeter at 30° C., 15-per-cent jelly) are suitable, in conjunction with other requirements such as those outlined above. The authors have tested over 40 different samples of glue from various suppliers, bought in accordance with a specification of this kind, and have found these requirements quite satisfactory.

The treatment of the glue during use is of no less importance than the quality of the glue itself. During the investigation it was frequently noticed that glue in jelly form showed rapid deterioration and loss in adhesive power. In Table III the results of tests of seven representative samples of glue are given. The breaking strength was determined in the same manner as described for Table I.

These tests show that a careful use of glue is at least as important as the selection of the glue itself and that continued or excessive heating is one of the worst forms of abuse.

Glue manufacturers recommend that their glue be not heated above a certain temperature (74° C. or 165° F.); but the danger of injury to the glue seems to be not so much in overheating as in continued heating to a moderate temperature.

TABLE III.—HEATING TEST OF GLUE.

Sample No.	Breaking Strength, (Average of 3 Tests), lb.	Viscosity at 30° C., 15-per-cent Solution (Scott), seconds.
1A.....	3086	30.0
1B.....	3126	29.0
1C.....	2310	25.5
2A.....	4303	34.5
2B.....	3943	25.5
2C.....	2370	23.5
3A.....	4630	44.5
3B.....	4763	37.0
3C.....	2780	33.0
4A.....	4236	43.5
4B.....	3403	39.0
4C.....	3083	30.0
5A.....	5163	44.5
5B.....	4696	43.5
5C.....	2386
6.....	4714	32.0
7.....	1933	24.5
8.....	5018	33.0

A.—Glue soaked 14 hours in cold water (1 part anhydrous glue and 5 parts water). Melted at 65° C. (149° F.) and then immediately tested.

B.—Sample A heated 4 hours to 65° C. (149° F.) and then tested.

C.—Sample B heated 20 hours to 65° C. (149° F.) and then tested.

As a general rule it should be insisted that glue be used within 24 hours of the first melting and be melted within about 16 hours of the first soaking. There are two kinds of deterioration to which glue is subject. When moistened or in the form of a cold or fairly cool jelly, there is deterioration due to bacterial growth (unless prevented by the addition of suitable preservatives) resulting in liquefaction and loss of viscosity. In the melted condition during heating there is a loss in what is commonly called "fiber strength" of the glue. The last-named

loss is the more serious of the two; for example, it takes but 4 hours' heating at 66° C. (150° F.) to reduce a grade No. 1 glue to grade No. $1\frac{1}{4}$, thus practically deteriorating it 11 per cent in value (see Table III) or 20 hours' heating at 66° C. (150° F.) to reduce it to grade No. $1\frac{1}{2}$, thus reducing it 22 per cent in value. This also emphasizes the necessity of keeping the conditions of soaking and heating of glue during test strictly uniform.

While the practical use of glue is not within the scope of this paper, the matter is nevertheless mentioned because of its bearing on the raw material. The quality of glue is frequently blamed for poor results when in reality it is the handling of the glue by the consumer which causes these results.

AN IMPROVED TYPE OF AXIAL STRAINOMETER.

BY ARTHUR C. ALVAREZ.

SUMMARY

The following tension and compression tests on timber, metal and concrete were made to illustrate the advantage of the author's elastic-deformation-measuring device for determining the true elastic limits of these materials. The tests indicated clearly that, in the application of so-called axial loading to such specimens, bending was always introduced regardless of the precautions taken to prevent it. The strainometer described below, in addition to measuring the axial deformation, enables the amount of such bending to be accurately calculated. The true elastic limit is then determined by properly combining the maximum stresses corresponding respectively to the axial and bending deformations.

It was found that more bending occurred in compression tests than in tension tests, and that the effects of such bending in different materials under compression increased with increased values of the modulus of elasticity. The error in determining the elastic limit of steel in compression may be as much as 32 per cent when the stress due to bending is neglected; for timber in compression, it may be as much as 11 per cent.

In tension and especially in compression tests, particularly of metals, irrespective of precautions taken to prevent eccentric application of loading, the amount of bending introduced by such eccentricity, which is nearly always unavoidable, should always be measured when it is necessary to determine the elastic limit very accurately.

The roller-dial micrometer, when equipped with rigid roller bars, has many advantages over other types of micrometers for making routine laboratory measurements.

AN IMPROVED TYPE OF AXIAL STRAINOMETER.

BY ARTHUR C. ALVAREZ.

In tension and especially in compression tests, it is practically impossible to apply loading concentrically so that no bending is introduced. Indeed, the continuance, as a test progresses, of even an initial concentricity depends largely on uniformity in resistance and in elastic behavior of the material throughout its cross-section, conditions which frequently do not exist in timber and concrete due to defects in the internal structure of these materials. Eccentricity also results from irregularities in the preparation of the bearing surfaces of compression specimens. Tests indicate that such irregularities are bound to occur in spite of the utmost care taken to prevent their presence. In the testing of metals in compression, the slightest variation from parallelism of the bearing surfaces of the specimen and the movable head of the machine produces a stress at the sides of the test specimen which is much greater than the average calculated by assuming the loading to be axial. The effects of eccentricity, as measured by increase in stress, are accentuated in metal tests because the modulus of elasticity of the material is large. In view of this it seems, to the author, necessary that the exact amount of additional stress due to such bending be considered in all precise determinations of the true elastic limit of a material subjected to direct stress. It is the purpose of this paper to describe very briefly a deformation-measuring device by means of which such fiber stresses can be determined.

The important principle of the instrument consists in measuring deformations along three gage lengths equidistant from the axis of the specimen and placed at angles of 120 deg. around a circumference about this axis. The average of the three deformations gives the axial deformation in the specimen. Inequalities between the deformations indicate bending and give sufficient data to calculate its exact amount.

Nearly all axial strainometers now in use consist of only two micrometers placed 180 deg. apart. It is true that the

mean of two such measurements gives the axial deformation in the specimen, because of their symmetrical position, and that any difference in the two micrometer readings indicates the presence of bending. With only two observations, however, there are not sufficient data to calculate the exact amount of

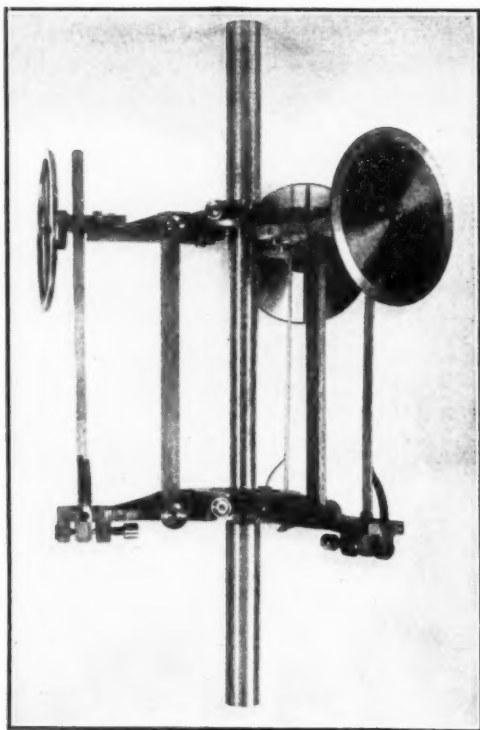


FIG. 1.—Strainometer Arranged for Tension Test of Metal.

bending, since the position of the plane of bending with respect to the line joining the micrometer centers is not known.

Following out the principle of the three-micrometer instrument, several types have been designed by the author for use in the Civil Engineering Testing Laboratory of the University of California, of which Mr. Charles Derleth, Jr., Professor of Civil Engineering, is director. These instruments are shown in

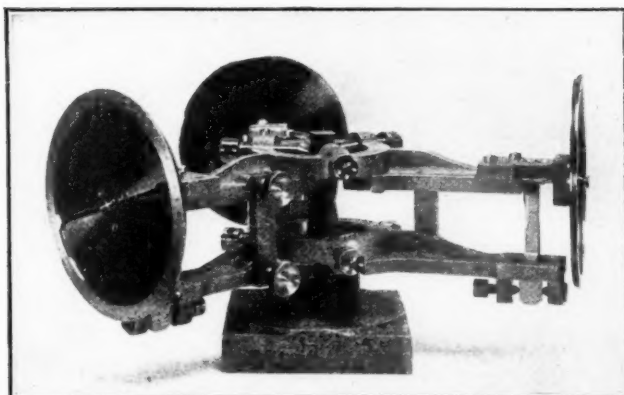


FIG. 2.—Strainometer Arranged for Compression Test of Metal.

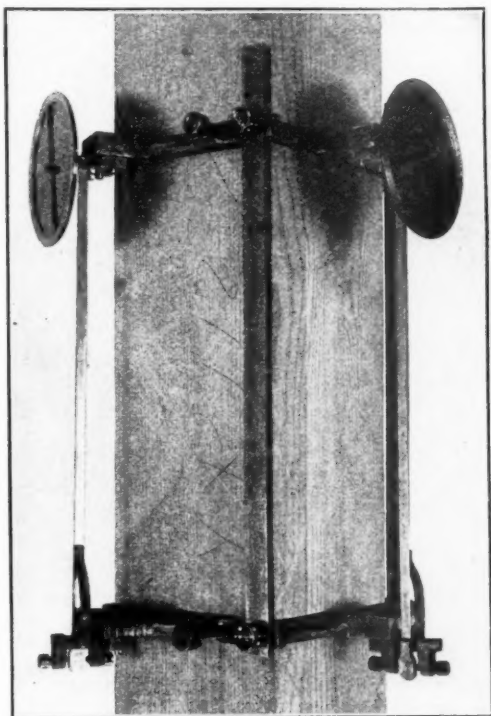


FIG. 3.—Strainometer Arranged for Compression Test of Timber.

Figs. 1, 2 and 3. Roller dials were used for reasons that will be given later. Three space bars hold the collars parallel, and at a known distance apart as they are being attached to the specimen. Each collar is attached by three contact screws placed symmetrically with respect to the dial gage lines. It is very necessary that the number of contacts per collar be not more nor less than three. If it be more than three, there is danger of straining the collar, if for any reason the plane section of the specimen defined by the contact points before loading is applied does not remain plane during the application of loading. Each dial with its roller and roller bearings is a unit. The dials are interchangeable between the several types of instruments so

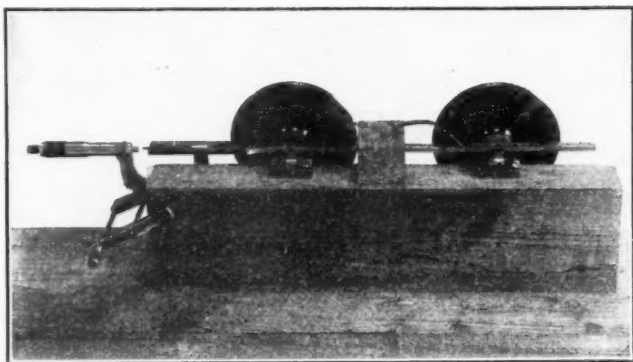


FIG. 4.—Strainometer Dials Arranged for Calibration.

that only three dials are required for making metal, concrete and timber tests. The instruments for compression and tension tests of metal are similar; the only difference is in respect to gage length. The dials are graduated from 0 to 500; the pointer has a vernier which indicates deformations to 0.0001 in. The circumference of the roller is 0.5 in. The rollers are made of hard steel so that there is practically no wear. Whatever wear does occur can easily be detected by calibration, which is very easily done. Deformations are communicated to the rollers through stiff aluminum strips pressed against them by springs, so that no slip can occur. The stiffness of the strips, together with the method of rigidly holding them between centers,

prevents any lost motion through this medium. In Figs. 1 and 3 the springs used to press the aluminum strips against the

TABLE I.—COMPRESSION TEST OF GRAVEL CONCRETE.

(SEE FIG. 5.)

Load, lb.	Dial Readings.			Measured Average Deformation, in.
	C	E	D	
2 000.....	0.0000	0.0000	0.1000	0.0000
5 000.....	0.0000	0.4998	0.1010	0.0003
8 000.....	0.0000	0.4996	0.1019	0.0005
11 000.....	0.0000	0.4995	0.1029	0.0008
14 000.....	0.0004	0.4996	0.1038	0.0013
17 000.....	0.0005	0.4996	0.1047	0.0016
20 000.....	0.0010	0.4998	0.1052	0.0020
23 000.....	0.0011	0.0000	0.1060	0.0024
26 000.....	0.0017	0.0004	0.1065	0.0029
30 000.....	0.0023	0.0010	0.1070	0.0034
33 000.....	0.0031	0.0014	0.1075	0.0040
36 000.....	0.0041	0.0022	0.1075	0.0046
39 000.....	0.0051	0.0028	0.1080	0.0053
42 000.....	0.0065	0.0037	0.1084	0.0062
45 000.....	0.0071	0.0043	0.1090	0.0068
48 000.....	0.0092	0.0049	0.1104	0.0082
51 000.....	0.0101	0.0058	0.1117	0.0092
54 000.....	0.0136	0.0062	0.1145	0.0114
57 000.....	0.0170	0.0070	0.1179	0.0140
60 000.....	0.0222	0.0080	0.1240	0.0181
63 000.....	0.0360	0.0100	0.1450	0.0303
64 100 (Maximum Load).....

TABLE II.—COMPRESSION TEST OF DOUGLAS FIR.

(SEE FIG. 6.)

Load, lb.	Dial Readings.			Measured Average Deformation, in.
	C	E	D	
1 000.....	0.0000	0.0000	0.0000	0.0000
10 000.....	0.0041	0.0027	0.0000	0.0023
20 000.....	0.0085	0.0059	0.0015	0.0053
30 000.....	0.0121	0.0086	0.0037	0.0081
40 000.....	0.0156	0.0113	0.0070	0.0113
50 000.....	0.0188	0.0139	0.0107	0.0145
60 000 (Elastic Limit).....	0.0224	0.0168	0.0149	0.0180
70 000.....	0.0262	0.0190	0.0188	0.0213
80 000.....	0.0304	0.0216	0.0227	0.0249
90 000.....	0.0351	0.0244	0.0263	0.0286
100 000.....	0.0400	0.0264	0.0312	0.0325
110 000.....	0.0468	0.0273	0.0355	0.0365
116 400.....	0.0540	0.0267	0.0401	0.0403
120 000.....	0.0601	0.0258	0.0448	0.0436
123 000 (Maximum Load).....	0.1030	0.4940	0.0580	0.0517

rollers were subsequently placed on the dial castings, so that their full pressure would be exerted near the rollers where it is most needed.

The dials may be very easily calibrated, two at a time, in the manner indicated by Fig. 4. A milled-steel strip is gently pressed against the rollers by the springs shown. This strip has a conical contact at its left end against which the flat-ended micrometer screw presses. The strip is also slotted, as shown, to allow the use of the ordinary type of micrometer caliper.

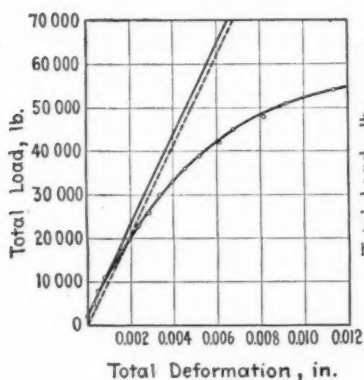
TABLE III.—TENSION TEST OF STEEL CALIBRATION BAR.
(SEE FIG. 7.)

Load, lb.	Dial Readings.			Measured Average Deformation, in.
	C	E	D	
1 000.....	0.0700	0.0000	0.0000	0.0000
4 000.....	0.0708	0.0008	0.0006	0.0007
8 000.....	0.0713	0.0025	0.0024	0.0021
12 000.....	0.0723	0.0040	0.0039	0.0034
16 000.....	0.0736	0.0052	0.0056	0.0048
20 000.....	0.0748	0.0066	0.0073	0.0062
24 000.....	0.0761	0.0079	0.0088	0.0076
28 000.....	0.0774	0.0095	0.0102	0.0090

TABLE IV.—COMPRESSION TEST OF STEEL.
(SEE FIG. 8.)

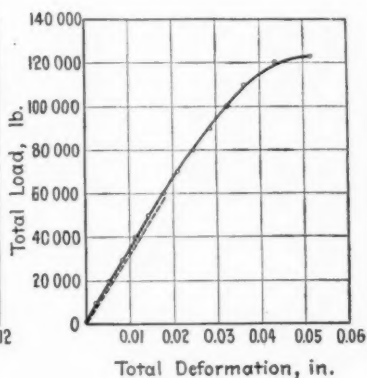
Load, lb.	Dial Readings.			Measured Average Deformation, in.
	C	E	D	
2 000.....	0.0600	0.0000	0.4000	0.0000
6 000.....	0.0592	0.0026	0.3955	0.0004
10 000.....	0.0592	0.0040	0.3929	0.0008
14 000.....	0.0594	0.0043	0.3919	0.0011
18 000.....	0.0597	0.0045	0.3908	0.0015
22 000 (Elastic Limit).....	0.0599	0.0050	0.3890	0.0020
26 000.....	0.0647	0.0037	0.3926	0.0028
	Increase = compression	Decrease = compression	Decrease = compression	

The dials are carefully adjusted by eye so that the axes of the rollers are perpendicular to the motion of the steel strip as it is pushed along by turning the micrometer screw. The screw-micrometer here used reads directly to 0.001 in. and can easily be estimated to 0.0001 in. Calibrations of the three dials in this manner indicated that none was in error by more than 0.0003 in. in a range of 0.08 in.



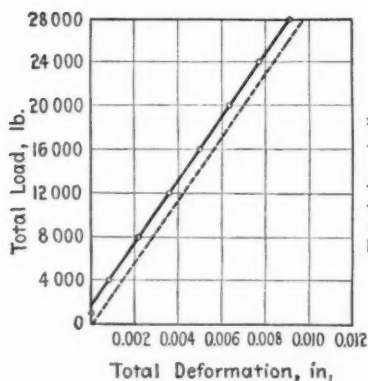
Proportions, 1c:3s:6g
Age, 1 year.
Specimen, 8x16-in. cylinder.
Gage Length, 13 in.
Maximum Load, 64 000 lb.
Initial Modulus of Elasticity,
2 640 000 lb. persq. in.

FIG. 5.—Compression Test of Gravel Concrete.



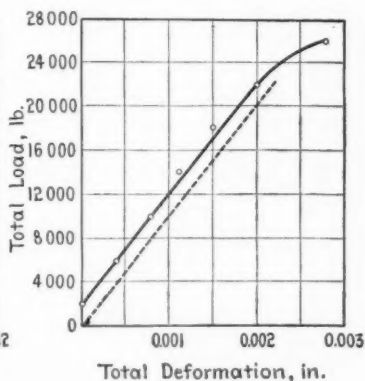
Air-Seasoned for 2 Years.
Size, 5.65 x 5.70 x 36 in.
Gage Length, 14 in.
Maximum Load, 123 000 lb.
Elastic Limit, 60 000 lb.
Modulus of Elasticity,
1 400 000 lb. persq. in.

FIG. 6.—Compression Test of Douglas Fir.



Gage Length, 8 in.
Diameter, 1.010 in.
Modulus of Elasticity,
28 900 000 lb. persq. in.

FIG. 7.—Tension Test of Steel Calibration Bar.



Gage Length, 2 in.
Diameter, 1.001 in.
Length, 3½ in.
Modulus of Elasticity,
25 400 000 lb. persq. in.

FIG. 8.—Compression Test of Steel.

LOAD-DEFORMATION CURVES FOR CONCRETE, TIMBER AND STEEL, USING THREE-DIAL STRAINOMETER.

Tables I to IV, inclusive, give individual dial readings for the various increments of loading as obtained from trial tests on concrete, timber and steel specimens. For additional descriptive data relating to the specimens mentioned in these tables, see Figs. 5 to 8, inclusive, which show the load-deformation curves corresponding to the average of the three observed deformations for each load.

In these tests great care was exercised to have the bearing ends of all compression specimens square and to make, by means of a spherically seated base plate, the top surface of each parallel to that of the movable head of the machine as the latter came in contact with the specimen. Nevertheless, an examination of the individual dial readings shows that the deformations along different gage lines were unequal in nearly all cases, that is, that more or less bending was always present. Moreover, the curves show that the average or axial deformation in the specimens followed the well-established laws for the several kinds of material.

The calculation of the amount of bending corresponding to three unequal deformations is a simple problem in solid geometry. Assume the three deformations for a load corresponding to the elastic limit to be a , b and c , increasing in the order named. Assume the plane defined by one set of collar-contact points to remain fixed and perpendicular to the axis of the specimen, and that the bending is due to the fact that the plane defined by the contact screws in the other collar becomes oblique to the axis of the specimen. In Fig. 9, let ST represent the fixed plane; pq , the axis of the specimen, od , nf and ri , the gage lines; ed , hf and ki , the deformations a , b and c respectively. Subtract graphically the deformation a , or the distance ed , from each of the three deformations, which leaves zero deformation at d , deformation gf at f and deformation ij at i . The problem then reduces to finding the angle between the plane ST and that defined by the points d , g and j . Now dgi is an equilateral triangle of known dimension; for metal $gi=8$ in., for timber, 9.7 in.; l is the intersection of the plane ST with ji produced; li can be calculated. Next solve triangle ldi for its altitude mf . These solutions can be made graphically very easily, quickly and with sufficient accuracy. The tangent of the angle required equals gf divided by mf . The tangent of this angle, multiplied

by the radius of the test specimen divided by the gage length, multiplied by the modulus of elasticity of the material as obtained by using average deformations, equals the intensity of stress to be added to the computed axial stress corresponding to the elastic-limit load, in order to obtain the true elastic limit of the material.

Considering merely the machine load at the elastic limit of the steel specimen tested in compression (see Table IV and Fig. 8) the elastic limit of the steel equals about 28,000 lb. per sq. in. Applying the bending correction as outlined above adds the relatively very large stress of about 13,000 lb. per sq. in. In

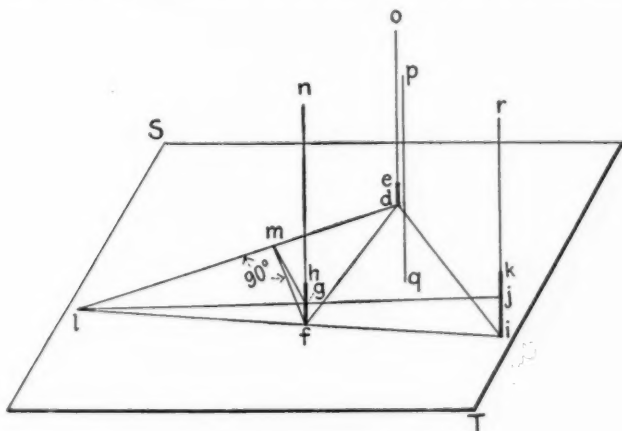


FIG. 9.—Solution for Flexure Angle.

other words, in this case, by neglecting the bending an error of about 32 per cent would have been made in determining the elastic limit for the metal in compression. For the steel calibration bar (see Table III and Fig. 7), at a machine load of 28,000 lb. the axial stress developed in the specimen equals about 35,000 lb. per sq. in. Applying the bending correction for a load of 28,000 lb. adds 650 lb. per sq. in. The correction for a tension test is much less, as would naturally be supposed. At the load corresponding to the elastic limit of the timber (see Table II and Fig. 6), an axial stress of 1860 lb. per sq. in. is developed. Applying the bending correction adds 230 lb. per sq. in. The error in determining the elastic limit of the timber

if the bending is neglected amounts to about 11 per cent. These results clearly indicate that, for accurate determinations of the elastic limit, the amount of bending introduced must be determined and allowed for in computing the resisting stress in the specimen. It follows, therefore, that the deformations must be read along three gage lines and that these gage lines must be equally spaced and symmetrically placed about the axis of the specimen in order that the axial deformation may be calculated for the purpose of determining the modulus of elasticity for use in the bending calculations. The elastic compression test of metal is one requiring very delicate manipulation. For this test a three-micrometer instrument should always be used.

Roller dials were used in place of screw micrometers for several reasons:

1. The dials are automatic. There is no handling required to obtain the reading. The mere turning of the micrometer, even if the contact is electric, might easily dislocate the collars when the micrometers are situated at a relatively large distance from the specimen as would be the case for metal tests.
2. The dial readings can be taken more rapidly.
3. Sudden failure in compression tests can do no injury to the dials or rollers.
4. The dials enable closer determinations of the elastic limit to be made, because any change in the rate of deformation, as an increment of loading is being applied, is immediately apparent by observation of the dial pointers.
5. To eliminate the subtractions otherwise necessary to calculate the deformations, the dials can be set at zero for the initial load.

It is sometimes stated that dials are unreliable because of slippage at the roller. In answer to this it may be said that, with proper roller axle bearings and springs to press the gage strips against the rollers, no slipping will occur. Slipping would cause a high value for the calculated modulus of elasticity. None of the values given in Figs. 5 to 8, inclusive, are high. The dials, however, should not be used on a testing machine that is subject to large vibrations when in operation.

DISCUSSION

MR. H. F. MOORE (*by letter*).—In connection with Mr. Mr. Moore. Alvarez' description of his "strainometer"—apparently an instrument of high precision—it may be pertinent to discuss the necessity of measuring the extension along each of three symmetrical axial gage lines in a tension test.

So far as the object of the test is to determine the modulus of elasticity, the writer can see no advantage of such an extensometer as Mr. Alvarez describes over an extensometer with two micrometers at 180 deg., or with one micrometer attached to a lever system which mechanically averages the elongation of the specimen. The modulus of elasticity depends on the *average* elongation rather than the *maximum*. If the object of the test is to determine the elastic limit (unit stress at first permanent set) or the limit of proportionality of stress to elongation, a knowledge of the distribution of the stress across the section of the test piece must be known if results of the very highest precision are to be obtained. Mr. Alvarez' statement that even in the most accurate laboratory tests there is considerable variation of stress across the section of a tension or a compression test piece is in accord with results published in England by C. A. M. Smith and also with results obtained in the Laboratory of Applied Mechanics of the University of Illinois.

There is one source of inaccuracy which may be present in an extensometer test which is not discussed in Mr. Alvarez' paper; inaccuracy in centering the extensometer on the specimen. The correctness of results obtained in a test with any extensometer—Mr. Alvarez' as well as any other—depends on the correctness of the assumption that the axial center line of the specimen coincides with the axial center line of the extensometer. Recent tests in the Laboratory of Applied Mechanics of the University of Illinois indicate that with ordinary centering by "eye," results of extensometer tests may be in error by as much as one or two per cent. The use of a template for centering any extensometer on the test piece seems to be necessary if high precision is to be obtained.

Mr. Moore.

The writer is not sure, however, that the additional refinement of measurement along three gage lines, as proposed by Mr. Alvarez, yields knowledge of much value as to the qualities of materials. In the first place, the precision of results obtained, as Mr. Alvarez has indicated, depends on the conservation of a plane cross-section by the test piece as it stretches. In view of the fact that the test piece is nearly always gripped at the outer surface, it would seem that there might be a variation of stress varying from surface to center as well as along a plane of bending, and that we could not count on absolute precision from any instrument which measured surface stretches only. In the second place, the physical meaning of either a limit of first set, or of a limit of proportionality, is not altogether clear. Such limits as are determined by static tests seem to be a function of the sensitiveness of apparatus used, rather than a fixed property of the material tested. Such limits do not indicate the dead-load strength of the material, and the endurance strength of material has been found at stresses lower than these statically determined elastic limits.¹

For the determination of static strength of a material the writer has found a carefully determined yield point, or the arbitrary "apparent elastic limit" proposed by the late J. B. Johnson² to be of greater practical significance than is the elusive "true" elastic limit or limit of proportionality.

The experience of the Laboratory of Applied Mechanics of the University of Illinois with friction roller dials (so long as they are free from scratches and minute flat spots) bears out Mr. Alvarez' statement that they are reliable. It has been found, however, under the hard service which most of the extensometers have been obliged to withstand, that such imperfections are very liable to develop and that the friction rollers eventually have become unreliable. In that laboratory the friction roller has been discarded in favor of a drum 1 in. in circumference, around which is wound No. 36 insulated copper wire.

Mr. Alvarez.

MR. ARTHUR C. ALVAREZ (*Author's closure, by letter*).—The writer has read Mr. Moore's discussion with interest. In what

¹ J. E. Howard, "Notes on the Endurance of Steels under Repeated Alternate Stress," *Proceedings, Am. Soc. Test. Mats.*, Vol. VII, p. 252 (1907).

² "Materials of Construction," p. 18.

follows, for convenience, he shall refer to the three-micrometer **Mr. Alvarez.** extensometer as the triple instrument, and to the two-micrometer extensometer, or its equivalent, as the dual instrument.

Near the beginning of the description, mention is made that the mean of the deformations indicated by a dual instrument gives the average stretch of the specimen. This of course assumes that plane normal sections remain plane during the application of loading, otherwise the mean of the two deformations is not equal to the average stretch in the specimen. It naturally follows, then, that the modulus of elasticity can be as accurately determined with the dual as with the triple instrument. But, the real purpose of the triple instrument is not to determine the average stretch in a test piece, but the maximum deformation. This maximum deformation, especially in compression tests, particularly of metals, frequently exceeds the average or axial compression by a relatively large amount.¹ This fact has particular bearing on accurate determinations of the elastic limits of metals in compression. The bending correction made possible by the use of the triple instrument is most important in metal compression tests. In such tests conditions usually are particularly favorable for very properly assuming the conservation of plane normal sections, because the bearing surfaces are milled, or planed off squarely in a lathe, so that every portion of the bearing areas receives compression directly. This makes the bending correction most reliable in the case in which it is most needed.

In tension tests, whether the ends are gripped by a threaded connection or with serrated V wedges, it is true that tension in the net section is caused by shear produced by the grips near the outer surface of the material within the grips. Under these conditions, due to the variation in longitudinal shearing deformation from surface to center of gripped portion of bar, variation by which tension is transferred to all fibers of the net section, the intensity of tensile stress so developed in the central fiber of the section might possibly be less than that in the surface fibers of the net section,—that is, plane sections might not remain plane for the case of a metal specimen subjected to tension in the usual manner. But, if this condition of non-

¹ See Table IV, p. 526.—Ed.

Mr. Alvarez. conservation of plane sections does exist to an appreciable degree in tension specimens of normal proportions, it can be reduced by reducing the sectional dimensions relative to the gage length and by increasing the length of grip. It is the writer's opinion that in ordinary elastic tension tests, the failure of plane sections to remain plane during the application of loading is not a serious matter when using a micrometer that reads only to 0.0001 in. It is to be noted, to be sure, that the bending correction is not nearly of the same magnitude and importance in tension tests as in compression tests because in the former it is easier to apply the load more nearly axially; but there is a correction, even for tension.¹

The use of a template for centering a delicate extensometer, as Mr. Moore mentions, is indeed very advisable. This can be done very readily. It was not done in the tests above mentioned. The resultant error, taking Mr. Moore's values at a maximum of 2 per cent, would be small compared with the errors that would have been made by neglecting the bending in the compression tests reported.

In conclusion, it is the writer's opinion that the results of actual tests, particularly of metals in compression, justify the use of a triple instrument when accurate values are desired for the elastic limit according to any of the accepted definitions of that variously considered quantity, whether it be the limit of proportionality between intensity of stress and corresponding rate of strain, or the intensity of stress for first permanent set, or the intensity of stress for 50 per cent increase in original corresponding rate of strain. Whatever the definition, it is a definite intensity of stress which represents the elastic limit. Even if the elastic limit as so determined—that is, specifically, though arbitrarily, according to one of the above conceptions—does not accurately indicate resistance of a material to either static or dynamic force, it is at least a quantity too well defined in most elastic materials to be regarded as an unimportant property. Its value to indicate ultimate resistance under any given conditions may properly be modified by empirically determined coefficients.

¹ See Table III, p. 526.—Ed.

A SIMPLE COMPRESSION MACHINE FOR TESTING STRUCTURAL MATERIALS.

BY WILLIAM O. LICHTNER.

SUMMARY.

The object of this paper is to present to the Society a compression machine of 125-ton capacity which is within the reach of small laboratories and construction jobs where large masses of concrete are used.

The machine is hydraulically operated by one man and registers accurately under small loads as well as under large loads. It has the following advantages over the machines found on the market at the present time:

- (a) High capacity;
- (b) Small initial cost;
- (c) Low cost to operate;
- (d) Small floor area;
- (e) Small weight.

The plates between the jaws of the machine are 10 in. square, which will admit a specimen 10 in. square, although this is larger than is customarily required. Ordinarily specimens are not apt to be over 8 in. square or 8 in. round by 16 in. high. The specimen 8 in. square can be tested to a strength of 3900 lb. per sq. in. and the 8-in. round specimen to a strength of 5000 lb. sq. in., which is in excess of the strength required for concrete.

A SIMPLE COMPRESSION MACHINE FOR TESTING STRUCTURAL MATERIALS.

BY WILLIAM O. LICHTNER.

With the increasing recognition of the necessity for laboratory tests of concrete has come a demand for a simple machine for testing the compressive strength of specimens as large as 8 and 10 in. in diameter. Engineers and builders are beginning to appreciate the fact that a great deal of money can be saved on a large concrete job, amounting often to thousands of dollars, by proper selection and proportioning of the aggregates. To compare the qualities and proportioning of different materials, it is necessary to make fairly large concrete specimens with the cement and aggregate to be used on the work. Advance tests are also essential from the standpoint of safety, and to determine the allowable working compressive strength of the concrete in a reinforced-concrete structure.

In addition to these tests of specimens made in the laboratory, it is now required on important construction that blocks of concrete shall be made up in the field at stated intervals, and tested for compressive strength at ages of say 7, 14 and 28 days.

In examining the catalogs and corresponding with the makers of testing machines for the purpose of purchasing for our laboratory a machine adapted to commercial and scientific tests of mortars and concretes, no satisfactory machine at moderate cost could be found. It was therefore decided to design a machine, which was done with satisfactory results at a cost of from one-third to one-half the price of a compression machine of the required size and weight.

Compression machines of from 100 to 150 tons capacity for testing concrete specimens would be in much greater demand were it not for their high cost and their bulkiness. There are two types of compression machines on the market: screw machines and hydraulic machines. For very large work the screw

machines are preferred to the hydraulic machines; but for small work, the former have the disadvantage of:

1. High cost;
2. Heavy weight;
3. Large floor area required;
4. Mechanical power necessary to operate.

The hydraulic machines now on the market lack some of the objectionable features of the screw type, being less in cost and in weight, requiring less floor area, and being generally operated by hand. However, they are still out of the reach of the smaller laboratories and of construction jobs where concrete specimens of 6 or 8 in. in diameter are made up for testing. The small hydraulic machines on the market at the present time within the reach of laboratories and construction jobs from the standpoint of cost and size, are not of sufficient capacity to test specimens of mortar or concrete larger than 3 in. or less in diameter. To obtain a good average sample of concrete, specimens should be cast at least 6 in. and preferably 8 in. in diameter. A machine to test specimens 8 in. in diameter must have a capacity of at least 100 tons. A capacity of 125 to 150 tons is still more preferable as affording a slight excess capacity and also providing for cases where it is desirable to test an old piece of concrete such as is sometimes cut from a wall. Such a piece even after sawing the faces true in a power saw is apt to be irregular in shape and may run a trifle over 8 in. in one dimension.

With a view to obtaining, particularly for tests of mortar and concrete, a reliable, accurate, and efficient machine of a capacity up to 125 tons, at a low cost, and requiring comparatively small floor space, the design shown in Fig. 1 was developed by our mechanical engineer, Mr. W. H. Weston, under the author's supervision.

This machine consists of an ordinary hydraulic jack like those used in ship-yard work, set in a frame consisting of a heavy base and top. The top of the frame is provided with a head block having a ball-and-socket joint with about $\frac{1}{2}$ -in. play in the joint, so as to adjust itself to the specimen and give it a square bearing.

To cover the range required by small specimens of mortar

and large blocks of concrete, two gages are required, which are calibrated to read the total pressure on the ram. The small gage registers up to 20,000 lb. by 100-lb. intervals, and the large gage registers up to 250,000 by 1000-lb. intervals, with possibility of interpolation for finer readings. Specimens of

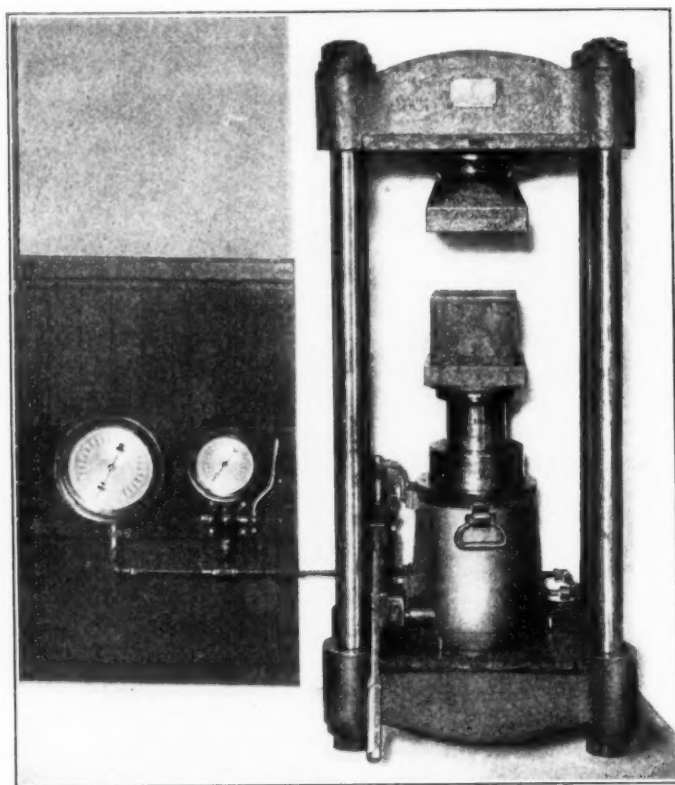


FIG. 1.—A Simple Compression Machine for Testing Structural Materials.

small sectional area will break under a comparatively small load on the ram, so it was desirable to be able to take readings by 100-lb. intervals which are read on the small gage. When the pressure on the ram exceeds 20,000 lb. the stop-cock on the small gage is turned off, thus preventing any further pressure coming

on this gage. This arrangement has been found to work very satisfactorily. Both gages are equipped with a "maximum hand" which consists of a wire held on the main stem of the gage hand by the spring in the form of a bent wire. This leaves the wire loose enough to be pushed around the dial of the gage, but not so loose as to be jarred out of position when the specimen breaks. The top end of the wire is bent outward so as to be caught by the gage hand as it moves around the dial when the pressure is applied. When the pressure is released, the gage hand returns to zero and leaves the maximum hand to indicate the pressure which was reached when the specimen broke. The gages have been carefully calibrated and register accurately at all points.

In order to protect the large gage from the sudden release of pressure occurring when the specimen breaks, it was found necessary to design a check valve to be inserted in the pressure line to the gage. This check valve consists of a small valve seated by a spring. When the pressure is applied, it forces the valve against the spring, allowing the water to pass into the gage. When a sudden release in pressure takes place, the spring seats the valve and the water in the gage gradually runs back to the pump by means of a very minute by-pass, which is in reality only a small scratch on the seat of the valve.

The jack is equipped with a single-stroke compound pump. The large plunger is used in raising the ram quickly to a height sufficient to tighten the specimen in the machine, and then this plunger is thrown out of service, which allows the small plunger to operate. The large plunger raises the ram about 0.08 in. per stroke, while the small plunger raises the ram about 0.02 in. per stroke. The load should be applied to the test specimen uniformly. This cannot be accomplished with quite the same uniformity in a hydraulic machine that can be obtained in a screw machine on account of the upward stroke of the pump handle. It has been found, however, that with care the pump can be so operated as to apply the load very evenly and without a jerk, and that by making a quick return stroke of the handle the pressure may be considered as being increased continuously. In applying the load, the ram should be raised at the rate of 0.026 ft. per minute, which can be done by operating the pump handle so as to make a stroke and return in $3\frac{1}{2}$ seconds. A

clock arrangement is being devised to strike every $3\frac{1}{2}$ seconds so that the man operating the machine can accurately regulate the pumping. When the load on the ram reaches the vicinity of 125,000 lb. or over, a pipe extension about 3 ft. long is put on the end of the 3-ft. pump handle. This gives the operator the necessary leverage to operate the pump without the assistance of another man.

A guard should be placed around the working parts of the machine to protect them and also the operator of the machine from injury when the specimens break. Concrete specimens having a height twice the diameter often break very suddenly, throwing small pieces of the specimen around in all directions.

The frame of the machine measures 20 by 32 by 60 in. high, outside dimensions, and the total weight is about $1\frac{1}{2}$ tons.

Net detail costs of this equipment complete with all attachments are approximately as follows:

125-ton 12-in. lift hydraulic jack.....	\$215.00
Tap jack for attaching gages and $\frac{3}{4}$ in. high pressure pipe.....	10.00
Frame complete.....	200.00
One 8-in. gage to register to 20,000 lb.....	20.00
One 10-in. gage to register to 250,000 lb.....	35.00
Check valve.....	5.00
Stop cock.....	25.00
Express and freight.....	25.00
Setting up.....	15.00
Total.....	\$550.00

The machine described has been in operation now for some time and has been found to give accurate and reliable results. The results have been checked several times by a series of check tests on a large expensive screw machine.

The author believes that there is a great need of a compression machine of 100 to 150-ton capacity, which combines the features of low cost, a small weight, small floor area, and small operating costs, and trusts the above description will be of assistance to those requiring a machine of this kind.

AN EFFICIENCY TESTING MACHINE FOR TESTING DRILLS, TAPS AND DIES.

BY T. Y. OLSEN.

SUMMARY

The advancement in the art of making high-speed cutting steel and the ever-increasing use of special cutting tools of such steel, have necessitated the development of special efficiency testing machines for determining their cutting quality.

It has been determined that various tool steels should be operated at a definite speed for maximum efficiency, depending on their treatment and the material they are to cut. It is essential, therefore, that this speed be determined by test and the machines either operated at a speed calculated to obtain the greatest efficiency from a given tool, or a steel obtained which will give the greatest cutting efficiency at a given speed.

The testing machine described in this paper has been found to successfully test the efficiency of drills, taps, dies, etc.

AN EFFICIENCY TESTING MACHINE FOR TESTING DRILLS, TAPS AND DIES.

BY T. Y. OLSEN.

Efficiency in production is the keynote in all well-managed manufacturing establishments and it is in no small measure the result of, and regulated by, tests of some description. It is controlled not only by methods of operation and design of special machinery, but also very largely by the kind and quality of the cutting tools to be used in a given operation.

The testing of materials has been developed mainly with the view of saving life or preventing injury, and at the same time of decreasing the quantity of material used to a minimum by raising its quality to a maximum. Testing has now broadened, however, and is being pushed into every phase of construction and manufacture.

In any manufacturing plant where metal is machined, there are two classes of tools used. One class comprises the fixed machines, whose quality, adaptability and use may be determined from inspection of design and construction. These tools are of a high first cost and are considered a part of the plant proper. The second class consists of cutting tools, and is small and insignificant when each tool is considered by itself. Such tools, however, are the real expense in operation, as the efficiency and output of any machine tool, no matter how well designed, is a function of the quality and form of the cutting tool. Such tools are quickly used up and must often be renewed. Their cost is large in the long run, but comparatively small when we consider what may be gained in production by the use of a high-speed cutting tool over the ordinary cutting tool of to-day.

This second class of tools comprises the following:

1. Tool steel, as used in lathes, shapers, planers, boring mills, etc.;
2. Files of all description;

3. Hack-saw blades;
4. Drills, taps, dies, and reamers;
5. Milling cutters.

Also under the list of tools of this description are included emery wheels and other abrasives, tests of which it is hoped to report at a later date.

All tools of this second class are purchased to-day largely through the efforts of salesmen, and as each salesman claims to have the very best, it is difficult to decide what to purchase. Usually, the sale goes either to the best salesman or to the lowest bidder. Also, purchasing agents in their efforts to serve well often buy only at the lowest price irrespective of quality. It should therefore be the object of any engineering department to specify such tests of material to be purchased, that the purchasing agent would know how to buy, and be able to obtain at the lowest price material subject to test.

The cutting property and efficiency of tools has been determined in some instances, but only by the very largest users, by means of tests of long duration at great expense. A machine that will give this information accurately and quickly at a minimum expense has been demanded, as it is just as essential for the smaller manufacturer to know the value of his tools and what he is purchasing, as it is for the larger ones.

The first tools of the above-mentioned list to be commercially tested were files, and such tools are to-day successfully tested all over the world by the Herbert File Testing and Indicating Machine. There are now over one hundred of these machines in use and they have proven unquestionably that files may vary greatly in quality. By their use, the proper cut of file for any class of material can also be determined; likewise the cutting property of metals. An attachment can also be obtained for this machine whereby hack-saw blades can be tested to destruction and curves obtained showing their quality.

The cutting property of tool steel has been determined in various ways, but until Messrs. Herbert and Fletcher, of England, devised their machine for the testing of tool steel, there were no ready means whereby such quality could be determined, except by actual practice tests, which were tedious and costly.

This type of machine is to-day used by some of the largest tool-steel makers and users in the world, and the results obtained are remarkable, in that they show not only the quality of the tool steel, but also the speeds at which such steel, properly treated, should be run to obtain the highest efficiency. By means of this machine actual cutting conditions are assumed in a manner which produces quick, accurate results at a minimum cost. The speed can be regulated and accurately determined, so that the cutting quality at any speed is obtained. From a series of such determinations the speed curve may be formed, which indicates perfectly the characteristics of the steel.

The testing of these various tools has led to the further desire to test such tools as drills, taps, dies and reamers. A machine for this class of testing presents a far more difficult problem than either of the other machines, since in this case it is necessary to test finished tools under actual working conditions. The machine must cover all conditions as to speeds and feeds, and must measure four variables, namely, pressure, torque, penetration and number of revolutions.

The machine to cover this class of testing is known as the Olsen Drill, Tap and Die Testing Machine, and as here described was designed to meet the precise specifications of the Philadelphia Navy Yard, where such a machine has been in successful operation for some time.

This machine, illustrated in Fig. 1, was designed to test drills, etc., from the smallest up to those 1 in. in diameter. To cover this large range of drills it was necessary to provide spindle speeds of from 50 to 1000 r. p. m., as well as a hand attachment for use in testing taps, dies, reamers, etc.

The feed problem resolved itself into two types; that of gravity or dead weight, and a positive feed.

The pressure measured is that which is exerted by the point of the drill on the material. It should be accurately obtained and in such a manner that it may be read off from an automatically recording dial at any time during the test.

The torque, which is the measure of the resistance to cutting, should, for accuracy, be measured by dead weight. This measurement must also be arranged so it will distinguish with

equal degrees of sensitiveness the variations in small as well as large drills.

The penetration is the measure of the depth of bore, taken directly from the motion of the cutting tool into the test material.

The number of revolutions required to give a certain penetration is also determined.

The machine consists of a bed on which a weighing head is mounted, at one end, which holds the material to be cut. At the other end is mounted a driving head with an arrange-

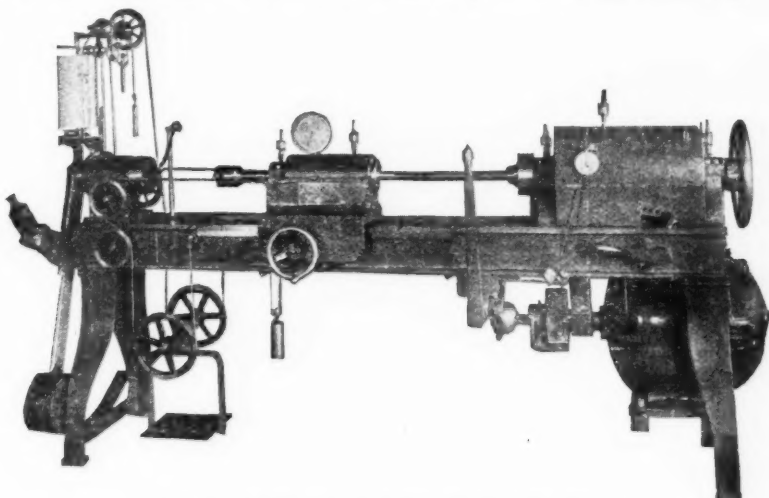


FIG. 1.—Drill, Tap and Die Testing Machine.

ment for changing speeds and positive feeds, etc. Between these two heads is a carriage mounted on ball-bearing rollers, supporting the spindle which carries the drills, etc., to be tested. The weighing head is bolted securely to the frame and in this head a pendulum, which measures the torque, is mounted in ball bearings. The pendulum is so arranged that by changing weights on it a maximum capacity of either 100, 1000, or 2000 in-lb. is secured. The moment or torque may be read from a direct reading, uniformly marked scale at any time during the test. Above the pendulum is mounted a large drum which by its rotation accurately records the torque on equally spaced

cross-section paper and to a largely magnified scale. To dampen the action of the pendulum an air piston is provided, which can be adjusted to suit requirements. Also, in cases where the drill may run temporarily above capacity or for some reason becomes stuck for an instant, the excess torque to an amount of 3000 in.-lb. can be measured on an additional scale.

The operating head rotates the spindle and is fitted with a changeable gear mechanism, and variable-speed motor drive, so that by one lever change any speed and motor control between 50 and 1000 r. p. m. can be obtained. In this head is located the positive feed mechanism, which can be thrown in or out of operation by a small lever, controlling a split taper nut on the spindle. This nut is given a rotation a fraction slower than the spindle speed, causing it to advance in proportion to the retarding of the nut. If both were to run at the same speed there would be no feed. The changes of feed are thus quickly made by shifting a sliding gear as indicated in the gear head, and a feed of either 0.01, 0.02, 0.03, 0.04 or 0.05 in. per revolution can be obtained.

The spindle is of vanadium steel and operates through a carriage as shown, which is free to move on the bed. In this carriage is mounted the dial indicator, which measures through the action of a hydraulic diaphragm the pressure on the point of the drill. The pressure is measured directly and automatically up to 5000 lb. In a more recently built machine of this type the pressure indicator has been mounted on the weighing head, where it is more directly under the eye of the operator, while testing.

The carriage is also mounted as noted above on ball-bearing rollers and through it the gravity feed may be applied by adding standard weights to the scale pan as shown.

A scale attached to the carriage indicates the actual penetration of the drill. The penetration may also be noted by the motion of a pencil on the rotating recording drum, thus giving the relation between torque and penetration.

The number of revolutions of the spindle may be noted on the dial in the gear head, or its relation to the torque automatically recorded on the autographic chart. A curve may also be obtained showing the relation between penetration and

number of revolutions, so that the machine will show graphically the relation between any two of the variables, torque, penetration and number of revolutions.

Both the weighing head and power head are arranged so that standard chucks can be used. The drills are usually placed in the power head and the material to be bored in the weighing head. In the case of die testing, however, the die is placed in the weighing head and the bar to be threaded in the power head and thus the bar, which may be of considerable length, can pass through the hollow spindle of the weighing head.

Ball bearings, both thrust and radial, are used throughout the machine. The bed is fitted with a trough and pump, and piping is provided so that any desired cutting compounds can be applied at the point of the drill and circulated, thus also affording a means of testing various kinds of such compounds.

In this machine every point has been considered so far as possible, so that any test can be made in which the cutting effort is concentric with the center of rotation cutting from a center. In addition to testing the cutting property of tools, it can also be used for determining the cutting property of steel and metals, the holding power of drill chucks, etc., and even for small autographic torsion tests.

A NEW VIBRATORY TESTING MACHINE AND RESULTS OBTAINED BY ITS USE.

By S. V. HUNNINGS.

SUMMARY.

It having been demonstrated that the radical variation in results of vibratory tests shown by specimens cut from bars or other material presumably uniform were mostly due to a variation in the rigidity with which the test specimens were held in the vibratory testing machine, the machine described in this paper was designed with a view to securing a machine that would hold all specimens absolutely rigid.

Other special features of the machine are as follows:

Rotative and reciprocating vibratory tests, as well as tension tests, can be made on the machine.

Specimens are tested under a direct tension load which can be accurately maintained throughout the tests.

The deflection can be regulated with accuracy and refinement and checked during the progress of the test without stopping the machine.

It permits the use of a test specimen that can be quickly prepared, and one that can be easily standardized.

Test specimens can be inserted or removed from the machine very quickly.

Although designed primarily for vibratory testing of stay-bolt iron, the machine has been built sufficiently strong to permit testing heat-treated alloy-steel specimens 1 sq. in. in sectional area.

The vibratory tests of various grades of iron and steel made on this machine, the results of which will be presented in connection with this paper, were made primarily to show the variation in the number of revolutions required to break specimens cut from the same bar or section of forging, but they also throw interesting light upon the influence of tensile strength, ductility and homogeneity upon the vibratory test results.

A NEW VIBRATORY TESTING MACHINE AND RESULTS OBTAINED BY ITS USE.

By S. V. HUNNINGS.

For a number of years several of the large railroad systems have included a vibratory test requirement in their specification for staybolt iron; other large consumers, while not actually purchasing iron under such a requirement, have given the property to resist vibratory strains marked consideration in placing their contracts. The great drawback against the inclusion of a vibratory test requirement in such specifications has been due to the radical variation frequently shown in the results of such tests, when made on specimens cut from the same bar, and apparently tested under identical conditions; and also the fact that no two vibratory machines, either built by makers of testing machines or by companies for their private use, were designed with the essential features sufficiently standard to permit of the adoption of a standard method of making a test that could be accurately adhered to for comparison with tests made on machines of different designs. These are the conditions which have confronted Sub-Committee III on Staybolt and Engine-Bolt Iron, of Committee A-2, in its effort to standardize this test.

A study of the machines and methods of testing by the above sub-committee indicated that the erratic results frequently obtained from specimens cut from the same bar and tested on the same machine, were due to the fact that the specimens, although apparently perfectly rigid in all cases, were held with different degrees of rigidity; furthermore, the root of the thread of bolts differed to such an extent in sharpness and smoothness as to exert a pronounced influence upon the number of vibrations the specimen would withstand.

It was with a view of securing a machine that would overcome these defects, and which would also permit making such tests rapidly with a high degree of accuracy, that Mr. C. L. Heisler, a designing engineer of the American Locomotive Co.,

developed a machine along lines suggested by the author's investigations conducted as chairman of the sub-committee referred to above. It is gratifying to be able to state that experience has shown the machine to possess the following advantages:

It is built especially strong; the heaviest-size staybolt iron, as well as large specimens of heat-treated alloy steels, can be tested as accurately as the smaller sizes of staybolt iron;

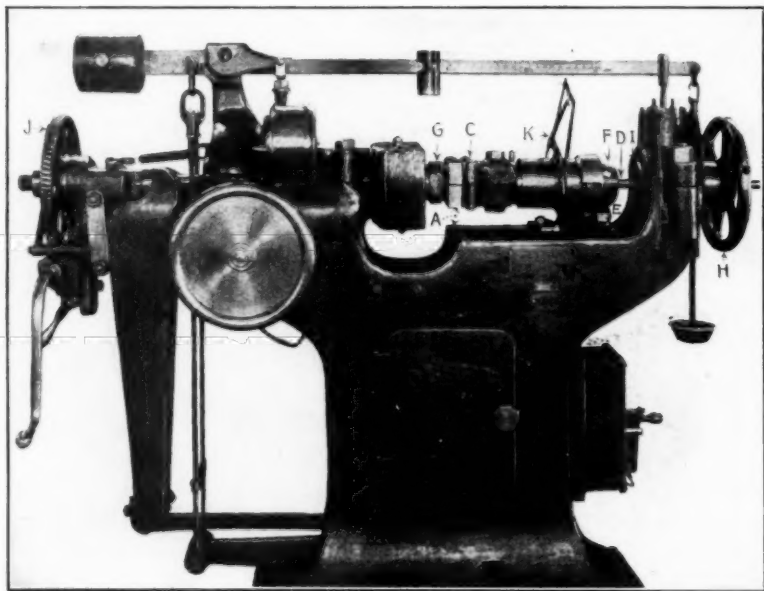


FIG. 1.—Vibratory Testing Machine.

the fixed end of the specimen is held absolutely rigid—the three-piece jaws and exposed position of the test specimen gives an operator or inspector ready means of satisfying himself of this fact; the accuracy of the tensile stress and deflection can be readily proven; the test specimen can be as easily placed and adjusted in the machine, and removed when broken, as a threaded tension specimen can be adjusted or removed from a tension testing machine; oscillating and rotative vibratory

tests, as well as tension tests, can be made on the machine, making it possible to subject round specimens to either an oscillating or rotative vibratory motion; furthermore, rectangular specimens, such as spring steel, can be subjected to the oscillating motion. To the author's knowledge no other machine possesses such a wide range of capacity.

Fig. 1 shows the machine adjusted for making a test by

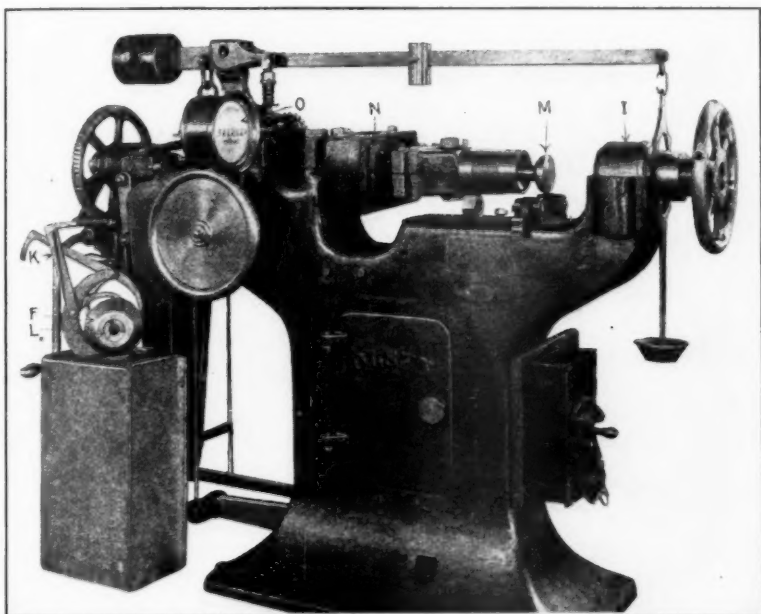


FIG. 2.—Vibratory Testing Machine, showing Details of Construction.

the rotative vibratory motion; to make a test by the oscillating motion it is only necessary to clamp a guide yoke at *A* and to adjust the bolt *C*.

Attention is called to the following features of the machine:

The bed or frame is made so heavy that a specimen of steel $1\frac{1}{4}$ in. in diameter and having an elastic limit of 125,000 lb. per sq. in., can be tested without causing the frame of the machine to weave a perceptible amount. The specimen may,

if desired, be tested under a direct tensile stress of 40,000 lb., and the load accurately maintained throughout the test. The weighing mechanism is as accurately designed as that of any tension testing machine; in fact, tension specimens of 2-in. gage length can be tested in this machine as accurately as in a tension testing machine.

The deflection is measured by a duplex multiplying caliper, one pair measuring the horizontal movement and the other pair the vertical movement. The measurement can be taken at any point from 6 to 12 in. from the fixed end of the specimen. Measuring the deflection in this manner insures against any inaccuracy, such as lost motion, due to wear in the eccentric bearing, which controls the amount of deflection or eccentricity.

To insert a specimen *D* in the machine, the dogs *E* are thrown up to engage the slots in the specimen chuck *F*; the chuck is then tightened by turning the nut *G*, the plunger wheel *H* is then adjusted and thereby tightens the chuck jaws *I*, which rigidly hold the fixed end of the specimen; by adjusting the wheel *J*, tension is applied to the specimen, and the machine may be started after taking the reading of the revolution counter *O*.

Fig. 2 shows some of the details of construction; namely, the forward specimen chuck *I*; the rear specimen chuck *F*, with the three-piece jaws *L* and multiplying calipers *K*; the ball-bearing end of the tension rod *M*; the wedges *N*, by which the eccentric bearing is adjusted; and the revolution counter *O*.

The jaws are chambered and fitted with circular leaf springs which throw them open when the specimen chucks *I* and *F* are released. The jaws first used on the machine were threaded in accordance with staybolt practice, that is, with twelve V-threads per inch. These jaws gave satisfaction and held the specimen perfectly rigid when the lead of the thread of the bolt was precisely the same as that of the thread of the jaws. The possibility, however, of errors occurring because of a very minute movement of the bolt at either end, the influence of which made the result of the test questionable, suggested the desirability of making the test on a more simple specimen, that is, a specimen that could be easily and quickly machined and be held more rigid than a threaded specimen; and finally, a specimen

having a form easily standardized, thereby insuring all tests being made under identical conditions. This has been repeatedly proven impossible when working on threaded specimens, as the bolts differed at the root both in sharpness and smoothness of the cut.

After considerable experimenting, it was found that the specimen shown in Fig. 3 was the most desirable from almost every standpoint, and that such a specimen could be held perfectly rigid on this machine with ordinary knurled jaws. This type of specimen and jaw was used in making the tests referred to in this paper. The specimen is well adapted for routine work, and is especially desirable for vibratory testing of stay-bolt iron where it is important that the diameter of the test section should closely approximate the diameter of the stay-bolt at the root of the thread. It is for this reason that the

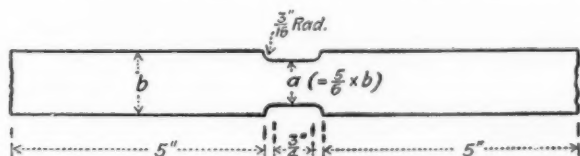


FIG. 3.—Vibratory Test Specimen.

diameter of the test section is made equivalent to five-sixths of the nominal diameter of the specimen tested.

For testing material not influenced by the amount of metal removed, and especially adapted for delicate work, experiments are being conducted with a different type of jaw. The results obtained to date are very encouraging.

The test specimen shown in Fig. 3 has the following advantages:

1. It can be prepared very quickly by roughing out with an ordinary lathe tool, finishing to size with a tool ground to a templet, then polishing with oil and emery paper. For closer work, the specimen can be roughed out, then ground to size. When testing staybolt iron, or other bars that are rolled close in size and truly circular, it is only necessary to center the specimen accurately and then machine the test section.
2. It is of a form that can be easily ground or polished and

will permit the ready use of a gage to check the diameter and radius of the fillet.

3. The length of the test specimen insures the concentration of the fiber stress at the forward end of the specimen, providing, of course, that the specimen is accurately machined.

4. A turned test specimen permits the study of the material during the progress of the test. This is especially interesting, and gives valuable information.

The tests shown in Table I (Plate VI) were made with a tensile stress of 5500 lb. per sq. in., and the specimen rotated under a deflection of $\frac{1}{16}$ in. as measured at a point 6 in. from the fixed end. When it was possible to do so, two vibratory tests were made from each bar or specimen; with but few exceptions the variation in results from the same bar have checked each other closely, as can be seen by reference to the table.

ADDENDUM (*by letter*)

While all of the vibratory tests included in Table I were made under the same conditions as to deflection and load per square inch, the results obtained from the bar-steel, cast-steel and forging specimens cannot be fairly compared with the vibratory results obtained from the staybolt iron, as all of the former specimens were rough-turned to a diameter of 1.015 in. before the test section was machined, while the only machining done on the staybolt specimens was the machining of the test section; as the 1-in. jaws, *L*, were machined a few thousandths full and as staybolt iron is specified slightly under size, the alignment of the jaws was not so true for the test of the staybolt iron as for the tests of the other materials. All staybolt tests, however, were made under similar conditions and show a degree of regularity, in specimens cut from the same bar, that to the author's knowledge cannot be approached by any other vibratory machine.

The series of vibratory tests of bar steel were made to show that the irregularity in vibratory results obtained from staybolt iron was due to the lack of homogeneity of such material and not caused by irregularities in the machine. For this purpose

TABLE I.—VIBRATORY TESTS MADE ON NEW OLSEN VIBRATORY TESTING MACHINE.

Serial No.	Description.	Vibrations (rotating).			Tension Tests.				Chemical Composition, per cent.						
		A	B	C	Tensile Strength, lb. per sq. in.	Yield Point, lb. per sq. in.	Elongation in 2 in., per cent.	Reduction of Area, per cent.	C.	Mn.	P.	S.	Cr.	V.	
STEELBOLT IRON.															
1	Brand A.....	10 930	9 350	12 620	48 260	33 810	28.12	45.3	
2	" A.....	11 800	9 440	9 450	48 620	33 780	32.50	50.2	
3	" B.....	8 050	7 720	8 830	49 930	35 160	30.87	50.8	
4	" B.....	10 610	13 680	10 070	49 910 ^a	34 400	33.00	50.8	
5	" C.....	9 760	7 900	8 410	49 830	34 220	31.25	48.6	
6	" C.....	8 940	8 510	9 480	48 090 ^a	33 750	31.12	54.0	
					50 680	36 980	30.25	48.6	
					50 430 ^a	36 140	30.87	53.0	
					50 830	37 820	30.00	51.1	
					50 670 ^a	37 760	29.62	52.1	
BAR STEEL.															
114	Bar A, untreated.....	7 190	6 310	7 460	48 660	35 620	45.0	72.8	0.09	0.26	-0.03	0.024	
115	" B, ".....	7 110	6 490	8 090	50 590	35 820	43.0	76.6	0.09	0.31	-0.03	0.025	
117	" C, ".....	4 660	4 570	4 670	61 690	38 380	36.5	64.6	0.125	0.59	-0.03	0.070	
118	" D, ".....	5 860	5 490	5 300	63 570	38 610	35.5	64.6	0.13	0.57	-0.063	0.080	
120 A	" E, ".....	7 290	7 640	6 390	73 680	43 410	31.5	33.3	0.38	0.55	-0.03	0.063	
120 B	" E, annealed 1450° F.....	4 000	4 140	70 960	42 600	32.5	56.0	0.38	0.55	-0.03	0.063	
120 C	" E, quenched and tempered.....	7 780	7 760	81 280	52 180	30.5	62.1	0.38	0.55	-0.03	0.063	
121 A	" F, untreated.....	8 780	10 800	76 590	42 170	31.0	57.9	0.20	1.05	-0.044	0.089	
121 B	" F, annealed 1450° F.....	6 900	75 400	41 080	31.0	48.4	0.20	1.05	-0.044	0.089	
121 C	" F, quenched and tempered.....	7 410	7 900	82 620	60 840	31.0	62.1	0.20	1.05	-0.044	0.089	
CAST STEEL.															
7	Carbon Open-hearth Steel (Acid).....	11 640	9 190	77 940	44 910	30.5	48.4	
8	" " " " ".....	10 730	11 230	77 940	44 910	30.5	48.4	
9	" " " " ".....	11 860	11 310	77 940	44 910	30.5	48.4	
10	" " " " ".....	11 350	11 840	77 940	44 910	30.5	48.4	
11	" " " " ".....	8 780	11 410	69 200	40 260	33.0	53.5	
12	" " " " ".....	10 450	8 820	69 200	40 260	33.0	53.5	
13	" " " " ".....	11 340	11 750	69 200	40 260	33.0	53.5	
14	Vanadium " " " ".....	19 200	80 940	48 060	26.0	47.8	
15	" " " " ".....	15 670	80 540	49 000	25.5	48.9	
STEEL FORGINGS.															
A-1	Chrome-Vanadium Cross-head Key, Quenched and Tempered.....	48 940	127 550	116 900	20.5	50.1	0.31	0.49	-0.03	0.03	0.96	0.16	
A-2	" " " " ".....	62 140	105 950	88 960	17.5	59.5	0.31	0.49	-0.03	0.029	1.01	0.17	
107	Carbon, B, untreated.....	8 820	7 000	87 670	53 070	21.0	27.0	0.56	0.33	-0.03	0.044	
108	" " annealed 1450° F.....	6 640	6 460	79 900	40 360	25.0	35.2	0.56	0.33	-0.03	0.044	
109 A	" " quenched 1550° F., drawn 1025° F.....	21 800	20 120	{ 14 400 20 320 }	114 800	72 200	17.0	37.1	0.56	0.33	-0.03	0.044	
109 B	" " quenched 1575° F., drawn 1300° F.....	11 600	11 010	95 620	62 840	23.0	46.0	0.56	0.33	-0.03	0.044	
110	" C, untreated.....	6 020	75 150	45 900	30.0	46.2	0.45	0.39	-0.03	0.039	
110	" " " " ".....	7 380	77 270	46 400	29.0	47.7	0.45	0.39	-0.03	0.039	
111	" " annealed 1450° F.....	5 070	5 160	4 770	68 620	36 830	30.5	49.3	0.45	0.39	-0.03	0.039	
112 A	" " quenched 1550° F., drawn 1025° F.....	11 360	12 060	91 150	56 180	22.0	34.4	0.45	0.39	-0.03	0.039	
112 B	" " quenched 1575° F., drawn 1300° F.....	6 970	7 390	80 200	50 430	27.0	45.3	0.45	0.39	-0.03	0.039	

NOTE.—All tension test specimens are those adjoining vibratory test specimen A, except those marked (a) which adjoin vibratory test specimen C.

six bars were secured from our stock material, two bars each showing approximately 50,000, 60,000 and 70,000 lb. per sq. in. tensile strength, respectively. The chemical analysis of the bars was not made until after the completion of the vibratory tests, when it was found that the difference in tensile strength of the different sets of bars was not entirely due to a difference in carbon content. In the untreated condition (as received from the mill), the vibratory tests of these bars showed greater regularity than shown by the staybolt specimens, but even greater regularity (although a lower figure) was obtained after annealing. A similar condition is also shown in the tests of the forgings.

The specimens from bars Nos. 114 and 115 show the highest vibratory tests of any of the untreated specimens. I think this was largely due to the fact that these specimens stretched radically during test and instead of breaking at the fillet, as was the case of all other specimens, they generally broke midway between the fillets, and as a rule developed two or more transverse cracks, which distributed the stress.

The specimens representing carbon open-hearth steel castings were tests cut from coupons that had been detached from castings and annealed separately in a small furnace. The vanadium cast-steel specimens were tests cut from coupons which were annealed while attached to castings.

The two tests representing a chrome-vanadium crosshead key were from the same forging, serial A-1 being cut along the edge, while serial A-2 was cut from the center. This is one case where a lower yield point and lower tensile strength gave the best vibratory figure. In this case, however, the reduction of area, chromium and vanadium were all higher in serial A-2, and may have had a bearing on the results.

Two carbon-steel forgings 6 by 6 by 60 in. were made, each forging being cut into three sections, one section of each being tested "untreated" (as it came from the hammer), one section of each was tested after being annealed, while another section was tested after being heat-treated, to give a high tensile strength, and then afterwards re-treated to give a much lower tensile strength and again tested. The results obtained from these tests show the following:

1. The results of the vibratory tests of the untreated sections of both forgings show noticeably higher than the vibratory tests of the annealed sections.

2. Annealing, while lowering the tensile strength and the vibratory strength, shows a pronounced improvement in the regularity of the two vibratory tests, as compared with the variations in the results of the vibratory tests of the untreated sections.

3. A comparison of the vibratory results obtained from serials Nos. 108 and 112 *B* indicates that there is not a great difference in the vibratory strength of an annealed carbon-steel forging and a heat-treated carbon-steel forging of approximately the same strength. In other words, when the strength and yield point are nearly equal, the higher elongation and reduction of area of a heat-treated forging does not increase the vibratory strength very materially.

4. The radical drop in vibratory strength of serials Nos. 109 *B* and 112 *B*, as compared to the vibratory strength shown by the same forgings Nos. 109 *A* and 112 *A* respectively, indicates strongly that high ductility was obtained at a heavy sacrifice of the vibratory strength.

The results of these tests are offered primarily to show the possibilities of this machine from the standpoint of routine vibratory testing, and also for work of investigation. Sufficient tests have not been made to warrant offering definite conclusions as to the significance of the results reported.

A MACHINE FOR TESTING CLAY PRODUCTS.

BY MONT SCHUYLER.

SUMMARY

It has been recognized for some time that the results obtained by the sand blast in the testing of clay products are as indicative of value as the more usual tests. The main objections to this method hitherto have been the difficulty of standardizing and the cost of the apparatus. By using centrifugal force in place of compressed air, a machine can be built which is free from these objections.

Tests on brick and plate glass, using the new machine, show that results are comparable, more easily obtained and reproduced, and, if anything, more reliable than the combined tests; particularly in respect to the grading of manufacturers and users.

Results so far obtained indicate that sands of widely differing origin may be used and that further experimentation will produce factors which will permit the adoption of this machine as a standard instrument.

A MACHINE FOR TESTING CLAY PRODUCTS.

BY MONT SCHUYLER.

The parallelism between rattler loss on paving brick and the indications furnished by the sand blast have been fairly well established by Mr. L. W. Page, Director of the Office of Public Roads. The only difficulty encountered by Mr. Page seems to have been that of standardization. Wear of nozzle, the trouble of keeping the air pressure constant, and the cost of equipment have made this method of testing "brick value" one not easy of standardization, and has prevented its wider acceptance.

This paper describes an apparatus (first proposed in 1911 by the author to Committee C-4) which generates the same destructive forces as the sand blast, but has the advantage of being easily and cheaply duplicated; and in addition a method of standardization is proposed which may make it possible to use sands of widely varying origin and character.

Figs. 1 and 2 show the general appearance of the first improvised machine. The motor and lower platform of a laboratory centrifuge furnish the base of the machine. The dust-tight cover has been roughly made from No. 16 gage sheet metal, and the rotating parts turned from shafting and seamless steel tubing. Subsequent machines could be made in a much simpler manner.

Two different sands have been tried on this machine, one being standard Ottawa sand, the other a crushed quartz passing a No. 8 sieve. There is quite a different rate of loss using these two sands, but it is probable that this is due more to the difference in grain size than to varying degree of sharpness. A close analysis of the losses on such widely varying materials as wood, hard-burned brick, vitrified brick, and glass seems to show that the indications furnished by the machine are the result of resistance to impact, rather than abrasion. For instance, plate glass suffers more than the slow-growth rings of yellow pine. Furthermore, the very decidedly over-burned

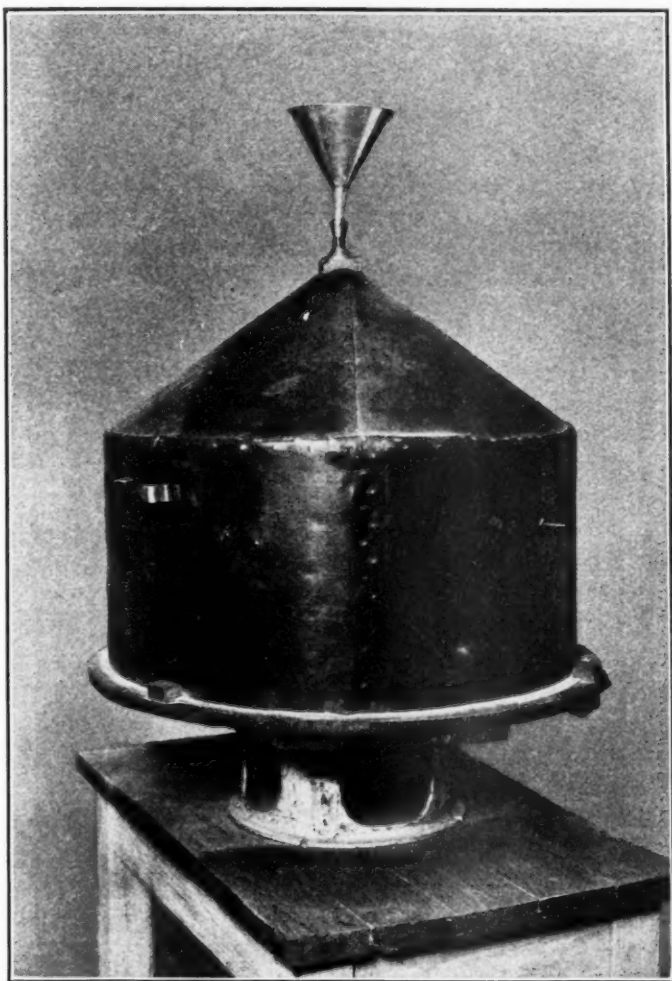


FIG. 1.—Centrifugal Sand-Blast Machine.

vitrified brick suffers a greater unit loss than a brick burned to the correct degree.

Fig. 3 shows the action of 10 lb. of the crushed quartz upon 6 dry-pressed bricks burned to various degrees.

The test rates them exactly as does the brick manufacturer and the absorption test, but not as the cross-breaking or compressive test, as may be seen from Table I. Fig. 4 illustrates

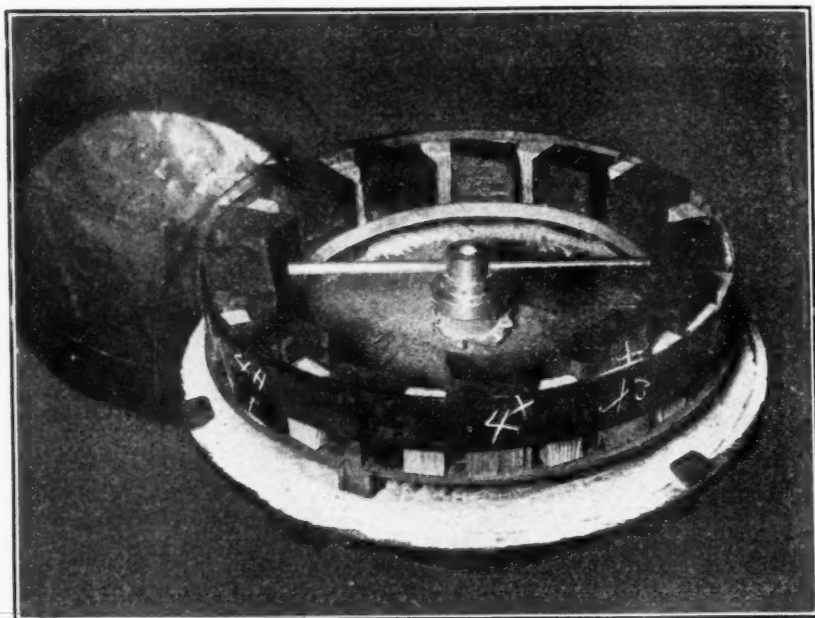


FIG. 2.—Centrifugal Sand-Blast Machine; Rotating Parts.

TABLE I.—ACTION OF CRUSHED QUARTZ UPON DRY-PRESSED BRICKS.

Manufacturer's Grading.	Abrasion Loss, g.		Modulus of Rupture, lb. per sq. in.	Compressive Strength, lb. per sq. in.	Absorption, per cent.
	1½ in. from end.	3 in. from end.			
Straight, hard.....	4.0	4.0	530	8400	5.1
Ordinary.....	9.0	9.5	830	4840	8.9
Medium No. 1.....	10.5	10.0	970	6740	10.0
Medium No. 2.....	17.0	17.5	830	4180	12.4
Light red.....	21.0	20.0	400	4740	14.2
Salmon.....	55.0	55.0	245	2240	18.1

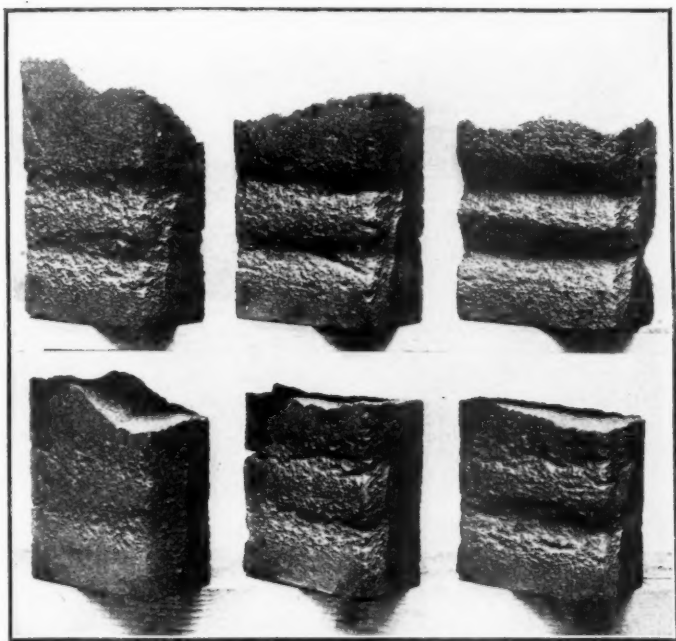


FIG. 3.—Typical Specimens of Hard-Burned Brick.

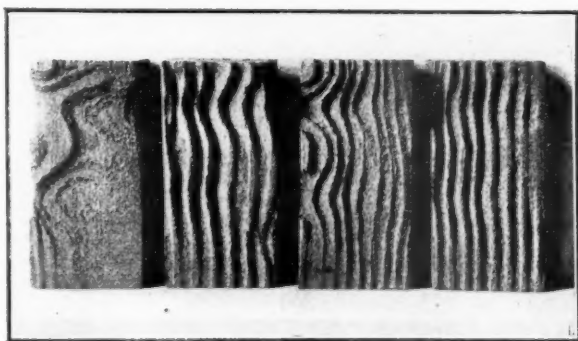


FIG. 4.—Yellow-Pine Spacing Blocks after Several Tests.

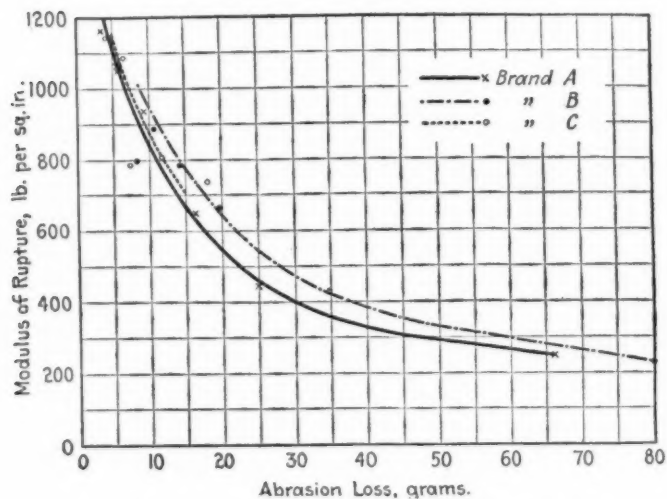


FIG. 5.—Abrasion Tests of Bricks, Plotted against Modulus of Rupture.

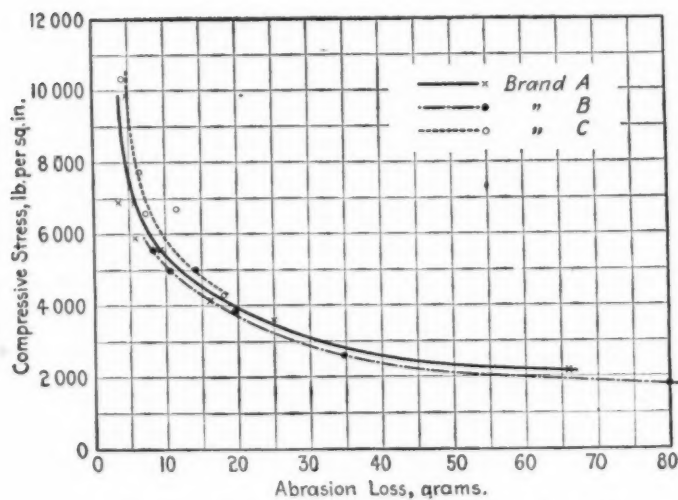


FIG. 6.—Abrasion Tests of Bricks, Plotted against Compressive Strength.

four spacing blocks of yellow pine after 15 or 20 tests, and perhaps suggests that this machine may be used in rating woods for hardness.

The curves shown in Figs. 5, 6 and 7 are the results of various tests made on brick from three sources, and are sufficiently clear not to need extended explanation. Each point is the average of 5 tests. When the piece-to-piece variation of clay products is considered, the curves must be held to be remarkably uniform.

An attempt has been made to standardize the machine against plate glass, which is probably of sufficient uniformity to

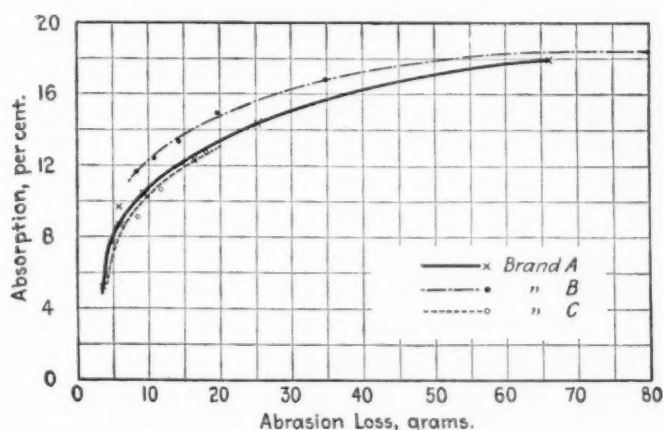


FIG. 7.—Abrasion Tests of Bricks, Plotted against Percentage of Absorption.

warrant its use for this purpose. Throughout the various tests the time required to feed 10 lb. of quartz sand was noted by means of a stop watch, and the number of grams lost by the plate glass were found to vary with the time of the test. Since a series motor is used in this machine, it is evident that the slower the feed the greater will be the speed of the motor and hence, the greater the velocity of the sand grains.

The phenomenon being probably that of impact, the conclusions are obvious.

To prove this point the rate of feed was reduced 70 per cent, with the result that the glass showed an increase in loss of 32

per cent, and brick, which had been tested previously at the usual rate, showed an increase in loss of 35 per cent.

In succeeding machines the motor, therefore, should be of the constant-speed type, and the rate of sand feed should be kept constant. By this means it is extremely probable that closely concordant results could be obtained.

A slight variation in the moisture content of the brick has no appreciable effect upon the impact losses. This, together with the fact that a test can be made in 15 minutes and without noise or dust, would make the apparatus of great convenience and utility in laboratories where clay products are under test.

TABLE II.—LOSSES OF PLATE GLASS, 1 BY 3 IN.,
EXPOSED TO ACTION OF SAND.
(TEN POUNDS OF QUARTZ SAND PASSING NO. 8 SIEVE USED.)

Time, seconds.	Loss, g.
60	2.1
62	2.2
62	2.2
65	2.4
61	2.2
60	2.1
60	2.2
59	2.1
204	2.9

An apparatus of this sort can be built for not over \$60 or \$70. It is portable and easily standardized and requires very little renewal beyond an occasional changing of the radial tubes at a cost of not more than \$1.

The author has not presented tables of data, for the reason that there is still much work to be done in standardizing various details and dimensions. The curves are indicative of parallelism with other well-known tests on clay products, and it is hoped that this work will prompt other investigators to undertake more extensive tests than have been found possible up to this time in the Municipal Testing Laboratory of St. Louis.

VISCOSITY MEASUREMENT AND A NEW VISCOSIMETER.

BY ALAN E. FLOWERS.

SUMMARY.

A brief history of the development of the conceptions of the resistance to fluid motion is followed by a discussion of the experimental methods which may be used for measuring viscous resistance, and the technical difficulties and errors inherent in each method.

Experiments are then described which illustrate the laws of the resistance encountered by a sphere rolling down a small slanted tube filled with a viscous liquid.

These experiments show that the resistance to the motion of such a sphere may be very great indeed, if the sphere nearly fills the tube bore; that it decreases at first very rapidly as the bore of the tube is increased and then more slowly.

It is shown that for a given ratio of diameters of sphere and bore of tube, the velocity attained increases more rapidly than the sine of the slope angle of the slanted tube, but the rate of change of velocity is the same for all viscosities above a certain minimum value, and that at any given slope the time to roll a given distance increases directly with the viscosity.

The results may be summarized as follows:

1. In general fluid resistance must be expressed by at least two terms

$$R = (K_1 S) V + (K_2 \gamma) V^2$$

where K_1 and K_2 are arbitrary coefficients; S = surface area; γ = specific gravity of the fluid; V = the velocity.

2. The term containing the first power of the velocity V gives the so-called viscous resistance and in general, in order to have the motion controlled by the viscous resistance, V^2 must be negligible compared to V .

3. The critical velocity of flow in tubes as determined by Reynolds does not apply to the very short outflow tubes used in the commercial flow types of viscosimeters.

4. The errors caused by exceeding the critical velocity for materials of low or moderate viscosity, the transition at some indeterminate point to conditions for viscous flow, the energy required for accelerating the fluid to the outflow velocity, the time required for making observations with viscous materials and the difficulties of temperature control and measurement in the commercial flow-types of viscosimeter, are large and variable.

5. A number of different methods of viscosity measurement have been discussed and results that may be obtained with the sphere rolling down a slanted tube described.

The conclusions that may be drawn from this work are:

1. The use of present commercial flow-type viscosimeters ought to be discontinued.

2. The sphere-and-tube type of viscosimeter may be used for comparison of absolute viscosity over a wide range with a moderate error.

3. Some of the advantages of the sphere-and-tube type of viscosimeter are:

- (a) Small amount of sample needed;
- (b) Protection from evaporation;
- (c) Protection from exposure to room temperature between check readings;
- (d) Short length of time needed for an observation;
- (e) Convenient and quickly made check readings by reversal of slope;
- (f) Smallness of correction for variations of specific gravity of sample;
- (g) Possibility of adjustment of observation time for very viscous materials by the use of denser spheres, higher slopes or a

shorter roll distance (the reduction of roll distance is the most convenient and accurate method to employ);

(*h*) Tests can be made at high temperature and under pressure;

(*i*) The sphere diameter should be from one-half to three-quarters of the diameter of the tube.

VISCOSITY MEASUREMENT AND A NEW VISCOSIMETER.

BY ALAN E. FLOWERS.

BRIEF HISTORY OF THE DEVELOPMENT OF THE LAWS OF RESISTANCE TO FLUID MOTION.

The subject of fluid resistance has a very respectable history, dating from the time of Gallileo. Gallileo,¹ who was interested in the laws of falling bodies, appreciated that even air would oppose motion, but as the effects were small for the falling bodies with which he was dealing it was sufficient for his purposes to consider the air resistance as a negligible correction.

Newton's² principal interest in the subject was in its connection with the laws of falling bodies. In book II, propositions 1, 2 and 3, he stated that the laws for the motion of bodies opposed by a resistance increasing directly as the velocity, but concluded with the statement that this form of resistance was more a mathematical hypothesis than a physical one. This was indeed true for the swift motions in which he was interested, as he proved by experiments on bodies falling in air and in water. He also later investigated the motion of the pendulum and calculated the damping on various mathematically assumed laws of resistance. Then he carried out a series of experiments to compare with the calculations. The results of his tests led him to express the resistance in a three-term equation of the following form:

$$R = aV + bV^{\frac{3}{2}} + cV^2$$

The last term, cV^2 , he found to give the greater part of the resistance, and found by comparison of the resistance to oscillation in air, water, and quicksilver, that this part of the resistance very closely, and the whole resistance approximately, varied directly as the density.

¹ "Œuvres de Gallilé," Edition de 1718, Vol 2, Florence.

² "Philosophiæ Naturalis Principia Mathematica," Edition 1687.

Newton took care to state that the law of resistance, varying as the density of the liquid and the square of the velocity, held only approximately, and for bodies moving swiftly. He also pointed out that tenacious fluids of equal density would offer more resistance than easy flowing liquids, as cold oil more than hot oil, warm oil more than water, and water more than spirits of wine. This is probably the first grading of liquids in the order of their viscosities.

To Coulomb¹ belongs the credit for putting the laws of the resistance of fluid motion on the firm basis of proved experimental fact. He employed for his tests, first a cylinder and later a disk, suspended by a fine wire and oscillating in the liquid to be examined. He very naturally carried out the first observations with water, and the first memoir describes a series of observations using a very sensitive suspension. These results for very minute velocities gave a resistance independent of the velocity, as follows:

$$R_0 = K_0$$

The magnitude² of the resistance so found corresponds to a figure 1.95×10^{-4} dynes per sq. cm., according to Potier, as quoted by Brillouin.^{3, 4}

Coulomb's next memoir describes the series of observations, made with a disk at higher velocities, and here he found the resistance much greater, so that the constant term $R_0 = K_0$ became negligible. Coulomb then proved, by using disks of different diameters and moving at different velocities, that the resistance was constant for unit surface and for unit velocity, or that

$$R_1 = K_1SV,$$

where S = surface area, V = velocity, and K_1 = a constant.

¹ Mémoires relatifs à la Physique, publiés par la Société française de Physique, Vol. 1; Mémoires de l'Institut (Savants Etrangers), Vol. 3 (1801); Histoire de l'Académie (1784).

² Bingham, *Physical Review*, Vol. 35, p. 407 (1912), describes what he terms "plastic resistance" as the resistance for zero deformation. It would be interesting to determine whether fluids have in general any measurable plastic resistance.

³ Viscosité des Liquides et des Gaz, Gauthiers Villars, Paris, Vol. 1, pp. 56, 135 (1907).

⁴ Guye, *Journal de Physique*, 2, Vol. 5, p. 620 (1912), in a research on the effect of temperature on internal friction of solids shows that at room temperature, the suspension of a body oscillating in a vacuum does damp the oscillation by absorbing energy when twisted and untwisted. This may account for Coulomb's result, provided Guye's results are free from error due to a small amount of gas friction.

Tests with different disk materials, even when greased so that water did not cling to the surface, or when greased and powdered with fine sand, gave the same resistance in water at any constant temperature. Hence he concluded that the resistance must be due entirely to the liquid and to be the result of internal friction of the particles.

Other tests in warm water and in oil showed similar results. The oil, however, gave a resistance 17 times that for water at the same temperature, 20° C.

Coulomb also made tests with the disks immersed to a considerable depth (60 cm.), and found the actual resistance the same as at 5 cm. depth, proving that moderate pressure did not affect liquid friction.

In Coulomb's disk and cylinder experiments there was no change of shape of the liquid, and no inertia effects due to the hurling aside of masses of water, but when he substituted two crossed rods, or one rod oscillating in an axial plane, these effects became appreciable and he was able to measure their values. This part of the resistance due to the inertia of the liquid Coulomb states to be proportional to the density, so that we can write the following equation:

$$R_2 = K_2 \gamma V^2$$

where V = the velocity, γ = the density of the liquid, and K_2 = a constant.

Coulomb found no appreciable variation of internal friction of water within the temperature limits of 12 and 20° C., but did find the internal friction of lamp oil not only about 17 times that of water at 20° C., but also subject to considerable variation with temperature.

We may conclude from Coulomb's researches that there are two main causes of fluid resistance, namely, internal friction or viscosity, and inertia. We may also conclude in regard to the viscous resistance that it is directly proportional to the *relative* velocity of the parts of the fluid, directly proportional to the surface involved, and independent of the pressure and of the surface conditions of the boundaries.

We may then put this relation for the viscous resistance in the form of the following proportion:

$$R_1 \propto \frac{SV}{d}$$

or as the equation:

$$R_1 = \eta \frac{SV}{d}$$

in which the constant η becomes the resisting force for unit area ($S = 1$) and unit velocity ($V = 1$) of planes, unit distance apart ($d = 1$).¹

Poiseuille,² Professeur de Physique à la Faculté de Médecine de Paris, became interested in the laws governing the flow of blood through the capillaries, and planned and carried out a series of measurements on the flow of water through small glass tubes, which for accuracy and conclusiveness of results well deserves to rank as one of the best pieces of experimental work ever done. Few of those who have worked on the subject since Poiseuille have attained the accuracy and consistency of results that distinguish his work.

It is well worth while describing some of the precautions taken. As Poiseuille was concerned with passages of very small diameter he selected very small bore tubes ranging from 0.014 to 0.0014 cm. in diameter. One set of tests was made with a 0.065-cm. tube. The bore diameters and section areas he measured with a microscope and checked by filling with mercury and weighing the mercury. The smaller tubes he thus filled and weighed repeatedly. These two methods gave results agreeing to within 1 part in 500. Poiseuille filtered the water 10 to 20 times till he could no longer perceive floating particles with his glass. The temperature was controlled by a water bath and kept constant to within 0°.05 C. Air pressure was employed to drive the water through the tubes, and correction was made for variation of water level, capillary forces and

¹ This does not imply that the actual force required to move a body through a fluid of unit viscosity is 1 dyne per sq. cm. of surface area if the boundary is 1 cm. distant and the velocity 1 cm. per second, but that this general relation forms the basis of the differential equations for the resistance due to relative velocity.

² *Académie des Sciences, Recueil des Savants Etrangers* (1842), Année (1846).

weight of air. The outflow was submerged so that drop-formation would not interfere with the regularity of flow.

Poiseuille's results proved that the amount of water discharged varied directly as the pressure and the time, directly as the fourth power of the diameter of the capillary, and inversely as its length.

Poiseuille's results may be stated in the form of the following ratio and equation:

$$Q \propto \frac{pd^4t}{L} \text{ or } Q = K \frac{pd^4t}{L}$$

where p = driving pressure, or the difference in pressure at the two ends of the capillary in dynes per square centimeter; d = diameter of the capillary in centimeters; L = length of capillary in centimeters; t = time of outflow in seconds.

The agreement of the test results with the above statement of Poiseuille's law was within 1 part in 500 until the tubes had been reduced to a small part of their original length.

Poiseuille made tests at 0.5, 10, 20, 30, 40, and 45° C., and found the same relations to hold, but with markedly larger values for the constant of discharge K at the higher temperatures.

Making use of the fundamental relations derived from Coulomb's work we can derive Poiseuille's law and evaluate the discharge constant K as Helmholtz¹ and Brillouin² have done. We then get

$$Q = \frac{\pi}{128 \eta} \frac{pd^4t}{L}$$

Corrections for the end effects due to the accelerating force required to overcome the inertia of the fluid and set it in motion have been developed by Neumann,³ by Hagenbach,⁴ and by Couette;⁵ and for the converging stream-line resistance by Brillouin.⁶

¹ *Wissenschaftliche Abhandlungen der Physikalisch-Technischen Reichsanstalt*, Vol. 1, p. 158 (1894); also "Collected Works," p. 223.

² *Viscosité des Liquides et des Gaz*, Gauthiers Villars, Paris, Vol. 1, pp. 56 and 135 (1907).

³ *Leçons de 1858-1859*, publiée après sa mort (1883); Jacobsohn, "Bericht der deutsche Naturforschungs Gesellschaft," p. 142 (1860); *Fortschritte der Physik*, Vol. 17, p. 76 (1861).

⁴ *Poggendorf's Annalen*, Vol. 109, p. 385 (1860).

⁵ *Annalen de Chemie et de Physique*, Vol. 6, pp. 21, 502 (1890).

⁶ *Viscosité des Liquides et des Gaz*, Gauthiers Villars, Paris, Vol. 1, pp. 56, 135 (1907).

The correction for end effect due to acceleration is equivalent to a pressure equal to

$$\frac{16 \gamma Q^2}{\pi d^2}$$

where γ = density in grams per cubic centimeter; Q = quantity of discharge in cubic centimeters; d = diameter of capillary in centimeters.

The correction for the resistance due to the converging stream lines at the entrance is equivalent to an additional length of tube equal to

$$\frac{\pi d \phi}{16}$$

where ϕ may be assumed as equal to 1 as a first approximation.

If we include these corrections and solve for the viscosity we get

$$\eta = \frac{\pi p d^4 - \frac{16 \gamma Q^2}{t^2}}{128 \pi \left(L + \frac{d}{16} \right) \frac{Q}{t}}$$

where p = pressure in dynes per square centimeter; d = diameter of capillary in centimeters; L = length of capillary in centimeters; γ = density of liquid in grams per cubic centimeter; t = time of outflow in seconds; Q = quantity of outflow in cubic centimeters.

Grüneisen¹ states, however, that the correction for acceleration becomes negligible when the discharge occurs under the surface, and hence there is no excuse for applying this correction to Poiseuille's results, although it has been done more than once.

Shortly after this (about 1851), Stokes² published a theoretical calculation of the viscous resistance offered by fluids to the motion of solid bodies of various shapes. Stokes was directly interested in the pendulum corrections due to air

¹ *Wissenschaftliche Abhandlungen der Physikalisch-Technischen Reichsanstalt*, Vol. 4, No. 2, p. 154 (1904).

² *Mathematical and Physical Papers*, Cambridge University Press, Vol. 3, p. 55 (1890).

resistance. One of the important cases solved was the resistance to the motion of a sphere falling through a viscous fluid of indefinite extent. He derived the following expression for the resistance:

$$R = 6 \pi \eta r = 3 \pi \eta d$$

Equating this to the force of gravity for a freely falling sphere he obtained for the terminal or limiting velocity:

$$V = \frac{2}{9} \frac{r^2 g}{\eta} (\gamma_s - \gamma_m) = \frac{1}{18} \frac{d^2 g}{\eta} (\gamma_s - \gamma_m)$$

where d = diameter of sphere in centimeters = $2r$; g = accelerating force due to gravity in dynes; η = absolute viscosity in dynes; γ_s = density of sphere in grams per cubic centimeter; γ_m = density of medium in grams per cubic centimeter.

This expression is very generally known as Stokes' law, and it has been used to determine viscosity, to determine the size of fog particles, and lately in determining the value of the unit electrical charge to calculate the retarding force on minutely charged spheres in an electric field.

Stokes used originally, instead of the absolute viscosity η , the ratio of viscosity to density, η/γ , but later corrected this himself.

Hagen,¹ a German physicist and engineer, published in 1854, an account of a number of tests he had made on the flow of water through fairly large tubes. These results were plotted in the form of curves with velocity at some given pressure as ordinates and temperature as abscissas. These curves showed a very remarkable effect in view of the (even then) well-known fact that the viscous resistance of liquids decreases with rise of temperature. At some point, occurring at lower temperature for greater pressures, the velocity not only ceased to increase with further rise of temperature but fell off, and after reaching a minimum began once more to increase but at a slower rate.

A satisfactory explanation of this critical velocity was not given until the appearance in 1883 of the work of Prof. Osborne

¹ *Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin, Math. Abt., D. 17* (1854).

Reynolds,¹ who carried out a series of tests on the flow of water through tubes. The first tests were made with glass tubes 54 in. long and having diameters of 1, $\frac{1}{2}$, and $\frac{1}{4}$ in., respectively. These tubes were arranged with bellmouths that were immersed in a large tank filled with water. Just at the entrance to the bellmouth a small tube was placed so as to discharge a minute stream of colored water into the bellmouth and so down the center of the glass tube. The stream of colored water could easily be followed with the eye as it flowed down the middle of the tube. A run was begun only after several hours rest so that the water in the tank had become absolutely quiet. The out-flow cock at the discharge end of the tube under test was then opened and water drawn through the glass tube. As the velocity of the water was increased the colored line in the center of the tube became thinner, but remained unbroken until suddenly a critical velocity was reached at which it broke into wavy lines, as seen by an electric spark, or a uniform discoloration as seen by steady light. This appearance of eddies was always sudden and always occurred far from the inlet. Further increase of velocity simply caused the point of break-up to occur nearer the inlet so that more of the tube was affected. Reynolds gave the following equation for the value of this critical velocity:

$$V_c = \frac{P}{43.79 d}$$

where V_c = velocity in meters per second; d = diameter of the tube in meters; P = ratio of viscosity to density.

The value of P was taken from the work of Poiseuille as

$$P = (1 + 0.0336793 \theta + 0.000221 \theta^2)^{-1}$$

where θ = the temperature in degrees Centigrade;

In c. g. s. units this equation becomes:

$$V_c = 12,730 \frac{\eta}{\gamma d}$$

where η = absolute viscosity in dynes; γ = density in grams per cubic centimeter; d = diameter of tube in centimeters.

¹ *Philosophical Transactions, Royal Soc.*, Vol. 174, p. 935 (1883); Vol. 186 A, p. 123 (1895).

Below this velocity the outflow obeyed exactly Poiseuille's law. Reynolds stated clearly that the *sudden* appearance of eddies was evidence that the previous condition of smooth motion must have been unstable. He thus was led to the conclusion that there might also be another and much lower critical velocity at which eddies already present would be suppressed.

A second set of tests was then carried out using new lead pipe 16 ft. long, $\frac{1}{2}$ in. in diameter in one case, and $\frac{1}{4}$ in. in diameter in the other. Water from a 4-in. city main was admitted through a rubber hose and cock, purposely constructed so that the water would turbulently enter the lead pipe under test. Holes 5 ft. apart and approximately 10 and 15 ft. from the inlet were drilled in the lead pipe and fitted with manometers. Measurements were made of water temperature, difference of static head, and rate of discharge. Small differences of head were read by cathetometer to 0.0001 in. Large differences were read directly by scales fastened to the manometers. The visual observation of the stream line could not be employed as in the previous case because of the initial turbulence of the water. In this case the relation sought was that between pressure-difference and velocity. So long as Poiseuille's Law is obeyed this relation is linear.

This set of tests was as conclusive as were the first ones, in bringing out a *sharply* marked *critical velocity*, at which initial turbulence was suppressed in the 10 ft. of tube between the inlet and the first manometer. The value of this critical velocity at which turbulence ceases was stated in the form of the following equation:

$$V_s = \frac{P}{278 d}$$

where V_s = velocity in meters per second; d = diameter of tube in meters; $P = (1 + 0.0336793 \theta + 0.000221 \theta^2)^{-1}$ in which θ = temperature in degrees Centigrade. In c. g. s. units this becomes

$$V_s = 2010 \frac{\eta}{\gamma d}$$

where η = absolute viscosity in dynes; γ = density in grams per cubic centimeter; d = diameter of tube in centimeters.

For velocities less than this critical value the velocity increased directly as the pressure-difference; that is, Poiseuille's law held exactly. At the critical velocity turbulence showed itself suddenly, making itself evident by the jumping of the meniscus in the manometers. This irregularity continued up to about $1.35 V_c$, when conditions again became steady and the velocity once more increased regularly with the increase of pressure, but not so rapidly as before. Thus a new régime sets in at the higher velocities which has been termed the hydraulic régime. Reynolds found that his experimental results, as well as those of d'Arcy, whose results he recalculated, were expressible in the form of the following equation:

$$\frac{Ad^3 i}{P^2} = \left(\frac{BdV}{P} \right)^n$$

where A = a constant = 67,700,000; B = a constant = 396.3; d = diameter of tube bore in meters; $P = (1 + 0.0336793 \theta + 0.000221 \theta^2)^{-1}$; θ = temperature in degrees Centigrade; V = velocity in meters per second; i = pressure difference in meters head per meter length of pipe;

$$n = \begin{cases} 1.732, & \text{for new lead pipe;} \\ 1.780, & \text{for old lead pipe;} \\ 1.820, & \text{for varnished surfaces;} \\ 1.790, & \text{for glass;} \\ 1.880, & \text{for new cast iron;} \\ 2.000, & \text{for incrustated cast iron;} \\ 1.910, & \text{for old cast iron after cleaning.} \end{cases}$$

For velocities less than the critical velocities,—that is, as long as Poiseuille's law is obeyed,—the exponent n is equal to 1 for all cases. This general equation for the resistance to the flow of water in pipes has then been proven to hold for diameters ranging from 0.00014 to 0.5 meters, and for a range of head from 1 to 700,000 with errors of the order of 2 to 3 per cent, with the exception of the limited range between V_c and $1.35 V_c$ where deviations of 10 per cent may occur.

Reynolds' results giving a critical velocity V_c at which eddies are suppressed, has been taken erroneously to mean that

below this velocity no eddies would occur even with short pipes. *This is not true.* Reynolds with the $\frac{1}{4}$ and $\frac{1}{2}$ -in. pipes had lengths of approximately 10 ft. (equal respectively to 240 and 480 diameters) in which no measurements were made, and suppression might have occurred at some point between the inlet and the first manometer at velocities less than the critical velocity given.

The Reynolds' critical velocity can not then apply to very short tubes such as those that have been used for commercial flow-type viscosimeters.

Shortly after the publication of Reynolds' work on critical velocity, commercial types of viscosimeters were devised which were intended to compare the viscosities of oils with the viscosity of water at 20° C. The principal types were the Engler¹ in Germany, the Redwood² in England, and the Saybolt³ in the United States. All these had fairly large and short outflow tubes, such that with the standard, water, the velocity would fall just below the Reynolds' critical velocity for suppression of eddies. In other words they were given as short an outflow time as possible in order to reduce the time required for observations on very viscous oils. Even with these proportions many viscous oils will require $\frac{1}{2}$ hour or more for one observation.

Recently Grüneisen⁴ has published curves for the limiting conditions for errors of 0.1, 1 and 5 per cent in the measurement of viscosity by capillary-flow measurements with submerged outlets. The results are based on calculations from Poiseuille's original test results supplemented by tests of his own. Grüneisen points out that the errors with which he was concerned were due to the eddy currents in the tubes in and near the entrance, and resistance in the converging stream lines leading to the entrance. The error due to acceleration was considered negligible, as the outlet was submerged and the velocity head converted itself into pressure head.

¹ Engler, *Zeitschrift der Vereines Deutscher Ingenieure*, Vol. 29, p. 882 (1885).

² Redwood, *Journal, Soc. Chem. Ind.*, Vol. 5, p. 128 (1886); "Petroleum," 2 Vols., London (1896).

³ Meissner, "Vergleichende Untersuchungen über den Engler'schen, Redwood'schen und bolt'schen Zähigkeitsmesser;" also *Chemische Revue*, Vol. 19, p. 30, Feb., 1912.

⁴ *Wissenschaftliche Abhandlungen der Physikalisch-Technischen Reichsanstalt*, Vol. 4, No. 2, p. 154 (1904).

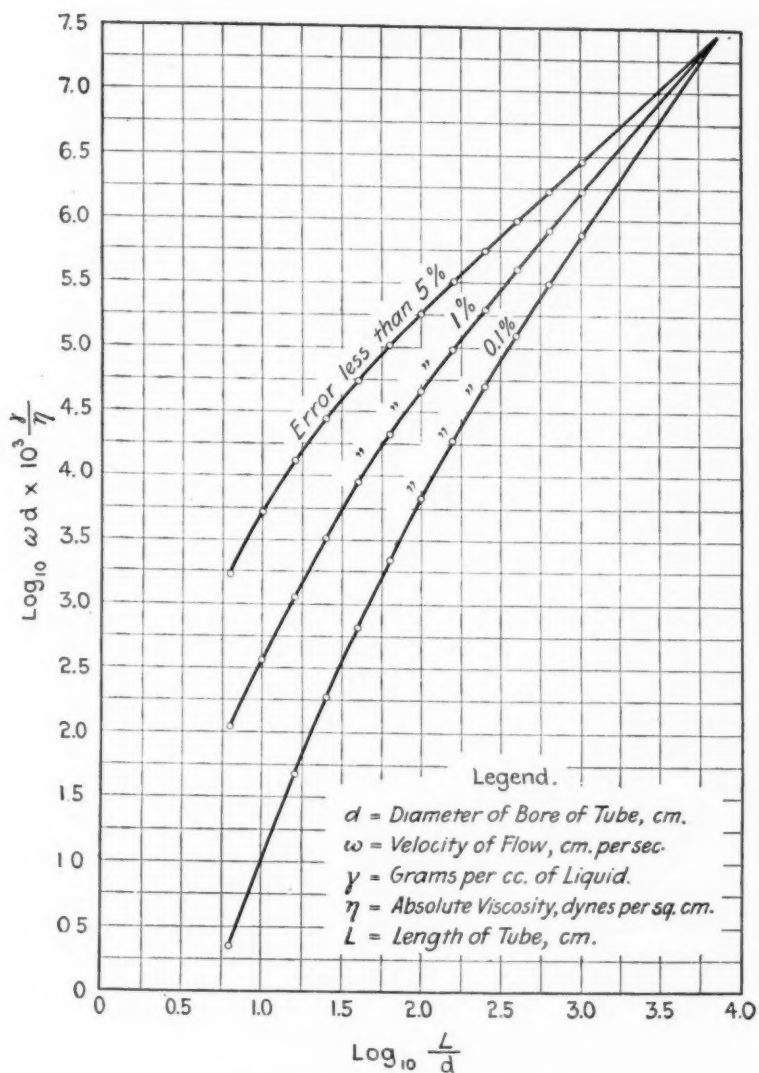


FIG. 1.—Limits of Applicability of Poiseuille's Law.
Calculated from curves of Grüneisen.

These curves are replotted in Fig. 1. It is interesting to note that the curves for different percentages of error converge, indicating a critical velocity at which the error increases at an indefinitely large rate. The intersection gives a critical velocity about double that given by Reynolds for the spontaneous appearance of eddies. The value taken from Grüneisen's curves extended is:

$$V_0 = 26,300 \frac{\eta}{\gamma d}$$

where η = absolute viscosity of liquid in dynes; γ = density of the liquid in grams per cubic centimeter; d = diameter of tube in centimeters. This critical velocity occurs for a length of tube $L = 7220 d$, or rather, for an indefinitely long tube.

In order to show more clearly the application of Grüneisen's curves as replotted in Fig. 1, the following concrete cases may be used:

$$\text{Let } \frac{L}{d} = 10, 100, \text{ or } 1000.$$

$$\text{Let the corresponding values of } \frac{\omega d \gamma}{\eta} = A_{10}, A_{100}, A_{1000}.$$

From the curves for 1-per-cent error:

$$\log A_{10} = 2.56 - 3 = -1 + 0.56;$$

$$\log A_{100} = 4.67 - 3 = 1.67;$$

$$\log A_{1000} = 6.22 - 3 = 3.22;$$

$$A_{10} = 0.363; A_{10}^{\frac{1}{10}} = 0.713;$$

$$A_{100} = 46.7; A_{100}^{\frac{1}{100}} = 3.60;$$

$$A_{1000} = 1660.0; A_{1000}^{\frac{1}{1000}} = 11.83$$

$$\text{and } \frac{\omega d \gamma}{\eta} = A \text{ or } \omega = A \frac{\eta}{\gamma d} \text{ is the limiting velocity.}$$

Now if the flow be caused by the head h due to the length of pipe L , and using the flow equation:

$$Q = \frac{\pi}{128 \eta} \cdot \frac{p d^4}{L}$$

where $p = h \gamma 981$ dynes, and h is the head in centimeters. Since $h = L$

$$p = L \gamma 981.$$

Also the quantity of discharge may be expressed by the equation:

$$Q = \omega t \frac{\pi}{4} d^2$$

Equating the two expressions for discharge, we have

$$Q = \omega t \frac{\pi}{4} d^2 = \frac{\pi}{128 \eta} \cdot \frac{p d^4 t}{L} = \frac{\pi}{128 \eta} \cdot \frac{L \gamma 981 d^4 t}{L}$$

and cancelling,

$$\frac{\omega}{4} = \frac{\gamma}{\eta} \cdot \frac{981}{128} d^2$$

$$\omega = \frac{4 \times 981}{128} \cdot \frac{\gamma}{\eta} \cdot d^2 = 30.65 \frac{\gamma}{\eta} d^2$$

Now equating to the limiting value of

$$\omega = 30.65 \frac{\gamma}{\eta} d^2 = A \frac{\eta}{\gamma d}$$

$$d^3 = \frac{A}{30.65} \left(\frac{\eta}{\gamma} \right)^2$$

$$d = \sqrt[3]{\frac{A}{30.65} \left(\frac{\eta}{\gamma} \right)^2} = \left(\frac{\eta}{\gamma} \right)^{\frac{2}{3}} \left(\frac{1}{30.65} \right)^{\frac{1}{3}} A^{\frac{1}{3}}$$

For water at 20° C., $\eta = 0.01006$ and $\gamma = 0.99823$.

$$\frac{\eta}{\gamma} = 0.01008; \left(\frac{\eta}{\gamma} \right)^2 = 1.016 \times 10^{-4}; \left(\frac{\eta}{\gamma} \right)^{\frac{2}{3}} = 0.0467$$

$$(30.65)^{\frac{1}{3}} = 3.13$$

$$d = \frac{0.0467}{3.13} A^{\frac{1}{3}} = 0.01492 A^{\frac{1}{3}}$$

$$d_{10} = 0.01492 A_{10}^{\frac{1}{3}} = 0.01492 \times 0.713 = 0.01063 \text{ cm.}$$

$$d_{100} = 0.01492 A_{100}^{\frac{1}{3}} = 0.01492 \times 3.6 = 0.0537 \text{ cm.}$$

$$d_{1000} = 0.01492 A_{1000}^{\frac{1}{3}} = 0.01492 \times 11.83 = 0.177 \text{ cm.}$$

For a 5-per-cent error, water at 20° C.:

$$\log A_{10} = 3.73 - 3 = 0.73; A = 5.37, A_{10}^{\frac{1}{3}} = 1.75$$

$$\log A_{100} = 5.27 - 3 = 2.26; A = 182.0, A_{100}^{\frac{1}{3}} = 5.67$$

$$\log A_{1000} = 6.46 - 3 = 3.46; A = 2880.0, A_{1000}^{\frac{1}{3}} = 14.2$$

$$d = 0.01492 A^{\frac{1}{3}}$$

$$d_{10} = 0.01492 \times 1.75 = 0.0261 \text{ cm.}$$

$$d_{100} = 0.01492 \times 5.67 = 0.0845 \text{ cm.}$$

$$d_{1000} = 0.01492 \times 14.2 = 0.212 \text{ cm.}$$

It is apparent from these results that the ratio of length to diameter should be large, that is, over 100; that the bore of the capillary tube must be very small to keep the error small; and that a very slight increase of diameter will greatly increase the error.

Archbutt and Deeley¹ have given a comparison of tests using glycerin solutions in several commercial viscosimeters with tests made by means of a long capillary of small diameter. The tests were actually carried out by mixing up a series of solutions of glycerin in water. As these solutions have densities greater than water and increasing at a variable rate with an increase of percentage of glycerin, the errors so found hold only for the particular values of absolute viscosity and density of the solutions used. These errors do not hold for thin oils which are lighter and but little more viscous than water.

Glycerin solutions are not well suited for comparative measurements, because slight variations in the percentage of glycerin or of its specific gravity cause large variations in viscosity, and because the glycerin solutions readily loose or absorb water from the air and change in viscosity.

Moreover we must conclude that there is a critical velocity for each ratio of length to diameter of outflow tube. This critical velocity is itself a function of the absolute viscosity and density of the liquid, and of the diameter of the outflow tube; which must lead to a discontinuity in the curves plotted between reducing factor and readings. Such a discontinuity is in fact visible in the curves given by Archbutt and Deeley.

As all these commercial flow-types discharge into the air

¹ "Lubrication and Lubricants," Griffin & Co., London, 3d Edition, p. 179 (1912).

the acceleration correction is not negligible but may be very large. Holde¹ quotes from Ubbelohde an expression for correcting the readings of the Engler viscosimeter. This correction is based solely on the acceleration, and consequently neglects the end effects considered by Grüneisen, and the critical velocity discontinuity just mentioned.

Variation of head during outflow is mentioned frequently as a cause of error in the commercial viscosimeter. If the outflow time were proportional to the viscosity there would be no error due to this cause, as can be proved from Poiseuille's data. The time would simply be proportional to the average head.

Batschinski² published in 1901 a comparison of the absolute viscosities of a large number of substances as measured by different observers with the following expression:

$$\eta = \frac{K}{T^3}$$

where K = a constant for any particular substance; T = absolute temperature in degrees Centigrade.

The constancy of the product of absolute viscosity and the cube of the absolute temperature proved that the law held within a fraction of 1 per cent for the majority of the substances considered. For water, alcohol, acids, the anhydrides, and in general for strongly dissociated liquids, the variation of viscosity is even *more* rapid with change of temperature.

Reckoning from a room temperature of 27° C., the substances that obey Batschinski's law will have a negative temperature coefficient of at least 1 per cent per degree, as may be seen from the following approximate calculation.

$$\text{Let } \eta = KT^{-3}$$

$$\frac{d\eta}{dT} = -3KT^{-4} = \frac{-3KT^{-3}}{T} = \frac{-3}{T} \eta;$$

$$\text{at } 27^\circ \text{ C., } T = 300$$

$$\frac{d\eta}{dT} = -\frac{3}{300} \eta = -0.01\eta$$

¹ "Untersuchung der Minerale Öle und Fette," Springer, Berlin, 3d Edition (1909).

² *Bulletin de la Société Impériale des Naturalistes de Moscou*, Vol. 15, p. 1 (1901).

The other substances have in general much larger temperature coefficients. The viscosity temperature coefficient for water at 27° C. is not far from -2.3 per cent.

Oils quite generally have an indefinite solidifying point. Near the solidification temperature the viscosity varies enormously with small changes of temperature.

In the course of this work one of the cylinder oils examined was found at room temperatures to vary in viscosity 70 per cent per degree Centigrade, difference of temperature from 20° C., and glycerin of specific gravity 1.2585 showed a variation in viscosity of 11.8 per cent per degree Centigrade from the value at 20° C.

Meissner¹ has recently published the results of viscosity measurements made with each of the three principal types of viscosimeter, the Engler, the Redwood and the Saybolt.

Gans² has determined the effect of reducing the time needed for an observation with the Engler viscosimeter by timing the outflow of a small volume of the liquid.

Eustace³ has shown very beautifully by means of glass tubes and colored streams of water that even moderate curvature in the outflow tube will disturb the flow lines appreciably and reduce the rate of discharge.

Hosking⁴ published in 1909 a very carefully carried out set of observations on the absolute viscosity of water throughout the temperature range of 0 to 100° C., using the capillary-tube method. His values are used throughout this work.

DISCUSSION OF THE METHODS OF MEASURING VISCOUS RESISTANCE.

We are now prepared to discuss the methods that have been used or proposed for measuring viscosity and draw some conclusions in regard to the difficulties of each method.

The methods may be classified as follows:

¹"Vergleichende Untersuchungen über den Engler'schen, Redwood'schen und Saybolt'schen Zähigkeitsmesser;" also *Chemische Revue*, Vol. 19, p. 30, Feb., 1912.

²*Chemische Revue*, Vol. 6, p. 221 (1899).

³*Philosophical Transactions*, Royal Soc., Vol. 85 A, p. 119 (1912).

⁴*Philosophical Magazine*, Vol. 5, p. 274 (1900); Vol. 18, p. 261 (1909).

- (1) Oscillation of a disk or a cylinder;
- (2) Capillary flow;
- (3) Rotation of a cylinder or a disk;
- (4) Free fall of a sphere;
- (5) Rolling of a sphere down a slanted tube;
- (6) Rotation of one of two concentric spheres.

1. *Oscillation of a Disk or a Cylinder.*—An example of the use of the oscillating disk or cylinder is given by Coulomb's apparatus. No entirely satisfactory general solution of the equations for the resistance to the rotation of the disk in the fluid has yet been given.¹

The damping, which is taken as the measure of the viscous resistance, is given by the difference of the amplitude of the swings, so that for small viscosities the value sought is obtained from the difference of nearly equal numbers. More than this, the result so obtained is subject to a large correction due to the air friction and the yield in the torsion fiber. This correction is determined by a similar set of observations making the final result the difference of a pair of differences.

The Doolittle² viscosimeter, in which a cylinder suspended by a torsion fiber is oscillated in the liquid to be tested, is subject to the same disadvantages as those already described for the oscillating disk.

2. *Capillary-Flow Measurement.*—Capillary-flow measurements carried out with long tubes having small diameters may be made to give very accurate results, as Poiseuille's work shows. It may indeed be considered as one of the standard methods for accurate scientific work, although few of those who have followed Poiseuille have succeeded in obtaining the accuracy and consistency of results that distinguish his work.

The causes of error and the precautions which must be observed may be considered under the following heads:

¹ Brillouin, *Viscosité des Liquides et des Gaz*. Gauthiers Villars, Paris, Vol. 1, paragraph 95-101 (1907); also Meyer, *Poggendorf's Annalen*, Vol. 113, pp 55, 193, 383 (1861); Vol. 125, pp. 177, 401 (1865); Vol. 143, p. 14 (1871); *Crelles Journal*, Vol. 59, p. 282 (1861). Maxwell, *Philosophical Transactions*, Royal Soc., Vol. 144, p. 249 (1866); König, *Weidemann's Annalen*, Vol. 32, p. 193 (1887).

² *Journal*, Am. Chem. Soc., Vol. 15, p. 173 (1893); *Journal*, Soc. Chem. Ind., Vol. 12, p. 709 (1873).

(a) *Critical Velocity*.—The critical velocity must not be exceeded nor approached. Here attention must once more be called to the conditions limiting the value of the critical velocity given by Reynolds.¹ These conditions are a length of pipe equal to 200 to 400 diameters for the suppression of eddies, and measurements of pressure taken along the pipe and not at the ends. For short pipes we should expect lower critical velocities.

(b) *Accelerating Forces*.—The force required to accelerate the liquid to the velocity in the pipe can easily be calculated, but it is better to make it negligible by having the outflow occur under the level of the liquid in the receiving vessel.

(c) *Resistance in the Converging Stream Lines at the Entrance*.—This resistance is due to the viscosity and its effect is to increase the total viscous resistance to outflow.

Brillouin² gives an expression for the equivalent length of pipe as

$$\lambda = \frac{\pi}{16} d\phi$$

and states that ϕ may be assumed as a first approximation to be equal to 1, but does not give the calculations. On this assumption,

$$\lambda = \frac{\pi}{16} d = 0.1962 d$$

and the equivalent length,

$$(L + \lambda) = (L + 0.1962 d)$$

is used in calculating the viscosity.

(d) *Eddies in the Tube near the Entrance*.—The resistance caused by eddies is a function of the density and of the square of the velocity, and not of the viscosity at all. This is the principal effect studied by Grüneisen.³ His curves make it possible to select conditions such that the error due to the

¹ *Philosophical Transactions, Royal Soc.*, Vol. 174, p. 935 (1883); Vol. 186 A, p. 123 (1895).

² Brillouin, *Viscosité des Liquides et des Gaz*, Gauthiers Villars, Paris, Vol. 1, p. 132 (1907).

³ *Wissenschaftliche Abhandlungen der Physikalisch-Technischen Reichsanstalt*, Vol. 4, No. 2, p. 154 (1904).

three causes, critical velocity, converging stream lines at the entrance, and eddies in the tube and near the entrance, may be kept less than 5, 1, or 0.1 per cent.

(e) *Surface-Tension Forces at the Two Surfaces.*—Surface tension exerts a force tending to raise the level of any liquid that will adhere to the walls of the containing vessel. For cylindrical vessels the head correction varies inversely as the diameter of the vessel. In the receiver where the level is rising and the surface rising to previously untouched portions of the wall, the surface-tension effect is irregular, and the best procedure is to make the receiving vessel of large diameter so that the effect on the head in this vessel will be negligible. In the tank which feeds the capillary, the amount to be subtracted from the measured head may be calculated if the tank is cylindrical; if, however, the tank is bulb-shaped, the average value of the correction must be determined by comparison with readings in a cylindrical manometer tube, as was done by Poiseuille.

The head due to the surface tension with cylindrical walls which have been wetted by the liquid is

$$h = \frac{2s}{g\gamma r}$$

where r = radius of the tube in centimeters; g = force of gravity in dynes; s = surface tension in dynes per square centimeter; γ = density of liquid in grams per cubic centimeter.

(f) *Selection of Capillaries.*—As the fourth power of the diameter of the tube appears in the calculations for viscosity, it is important to select tubes that are very nearly circular in cross-section and uniform in diameter from end to end. It is then necessary to measure the diameter very accurately.

Fisher¹ described recently a graphical process for averaging the reciprocals of the results of measurements of the length of a drop of mercury as it is moved along the tube. This average and the volume of the tube, as given by filling with mercury and weighing, are used to determine the effective diameter.

¹ *Physical Review*, Vol. 24, p. 385 (1907).

Considerable skill and time are required for this part of the determination.

(g) *Temperature Control and Measurement.*—The whole apparatus, supply tank and capillary, must be brought to a uniform temperature and kept constant at this temperature for long periods of time. As a temperature difference of $0^{\circ}.1$ C. causes a variation of viscosity of at least 0.1 per cent, errors in temperature control and measurement are probably the greatest source of experimental error in comparative measurements of viscosity.

(h) *Length of Time Required.*—When testing the more viscous materials, if the outflow conditions are kept the same as for water or for thin oils, single observations may require periods of $\frac{1}{2}$ hour or more.

In considering commercial examples of viscosimeters we are confronted by all the difficulties mentioned above in an exaggerated form. Since they are usually standardized with water at 20° C., the critical velocity with water is exceeded and the resistance when so standardized may be made up of five-sixths eddy current and acceleration resistance, and one-sixth viscous resistance.

If we use Coulomb's general equation for the resistance to fluid motion,

$$R = R_0 + R_1 + R_2,$$

but neglect R_0 in comparison with R_1 and R_2 , the equation may be written as

$$R = K_1\eta sV + K_2\gamma V^2$$

We then get the ratio error to be:

$$E = \frac{K_1\eta sV + K_2\gamma V^2 - K_1\eta sV}{K_1\eta sV} = \frac{K_2\gamma V^2}{K_1\eta sV}$$

or

$$E = \frac{K_2\gamma}{K_1\eta} V$$

where γ = density in grams per cubic centimeter; η = absolute viscosity in dynes; V = velocity in centimeters per second.

The last expression shows that the error varies directly as the density and the actual velocity, and inversely as the

true viscosity. Add to this the discontinuity in passing the critical velocity as the viscosity increases, and it is readily seen that the correction is difficult to calculate. Comparison with water may give ratios that are as much as 500 per cent in error.

End effects must be relatively large because the length of the outflow tube is seldom even 10 times the diameter.

The driving force is that due to the head and the density of the liquid, so that the driving force not only varies during an observation but varies in different liquids according to their densities. The correction required for comparison of liquids of different densities, though large, could readily be made if they were all obeying the laws of viscous flow. This correction is seldom made, however. Density moreover not only affects directly the eddy and end errors, but also the critical velocity, so that variations of driving force due to variations of head may cause the velocity to pass through the critical value in the course of a single observation.

As the outflow is usually to a vessel exposed to room temperature, cooling may occur during outflow, as well as drift of temperature during the period required for an observation.

Expansion of the outflow tube, if composed of copper, would cause an error of 1.25 per cent at a rise of 300° C.

Most of the sources of error in flow-type viscosimeters have been investigated *separately* by different observers. Generally each observer had in view the object of determining correcting factors for one source of error. It is the object of this discussion to show (1) that all of the sources of error must be considered together, and (2) that the resulting error is so large and complex that the use of the present flow-types of viscosimeters should be abandoned altogether.

In Fig. 2 are shown the results of tests of an Engler viscosimeter using the standard material, distilled water. The viscosity was varied by changing the temperature of the water. The change in reading (time of outflow) by no means corresponds to the change in viscosity. At 20° C. the change is indeed only about one-sixth of the value it should have, so that about five-sixths of the outflow resistance is due to other causes—inertia, eddy currents, etc.

The relations of relative viscosity and temperature were taken from the values for the absolute viscosity of water given in 1909 by Hosking.¹

3. *Rotation of a Cylinder or a Disk.*—The rotation of a cylinder has been used by Couette² and by Drew.³ Both obtained fairly satisfactory results but found some difficulty in keeping

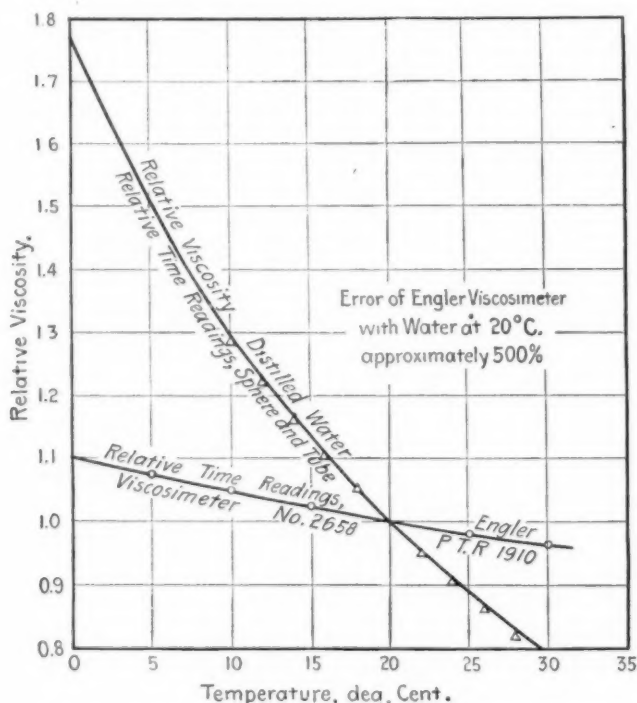


FIG. 2.—Comparison of Standard Engler Viscosimeter and Time of Roll of Sphere down a Slanted Tube.

the inner rotating member equidistant from the outer vessel. This position gave minimum resistance but seemed to be unstable. Some means for eliminating or minimizing the effects of the ends is also very desirable. This may be done by guard cylin-

¹ *Philosophical Magazine*, Vol. 5, p. 274 (1900); Vol. 18, p. 261 (1909).

² *Journal de Physique*, Vol. 9, p. 414 (1890).

³ *Physical Review*, Vol. 12, p. 114 (1901).

ders near to and in line with the stationary cylinder, or by extended rings.

The rotation of a disk has been used, though principally for the measurement of the viscosity of gases. The mathematical theory of the resistance offered by a fluid to the rotation of a disk is not yet in satisfactory shape.

4. *Free Fall of a Sphere.*—Most of the work on this variety of motion has been the result of recent attempts to measure the velocity of small particles,—particularly electrically charged particles,—in the determination of the value of the unit quantity of electricity. Consequently most of the work has been directed towards finding the limiting conditions for the application of Stokes' simple law.

Ladenburg¹ has given a theoretical calculation of the correction for the effects of the walls and ends of a cylindrical containing-vessel on the motion of a sphere along the axis, as well as experimental confirmation of the calculated correcting factors.

Ladenburg's expression for the final velocity is

$$V = \frac{2}{9} \frac{r^2 g}{\eta} \frac{\gamma_s - \gamma_m}{\left(1 + 2.4 \frac{r}{R}\right) \left(1 + 3.18 \frac{r}{h}\right)}$$

where η = absolute viscosity in dynes; γ_s = density of the sphere in grams per cubic centimeter; γ_m = density of the liquid in grams per cubic centimeter; R = radius of the vessel in centimeters; r = radius of the sphere in centimeters; g = force of gravity in dynes per second; h = height of the vessel in centimeters.

The above expression holds for a sphere of radius r in a vessel of height h when the sphere moves through the middle third of the vessel's height. Note that Ladenburg gave $3.3 r/h$, but recalculation using his method gave the value 3.18 for the coefficient of r/h .

Arnold² has extended the experimental work and shown that the expression given by Ladenburg holds up to the ratios of $r/R = 0.1$ with an error less than 0.5 per cent.

¹ Inaugural Dissertation, München, *Annalen der Physik*, Vol. 22, pp. 202-287 (1907).

² *Philosophical Magazine*, Vol. 22, p. 755 (1911).

A lower limit to the velocity and the size of the sphere is given by the condition that allows what corresponds to slippage at the surface. One necessary assumption in Stokes' derivation is that the fluid adheres to the sphere, or that there is no relative motion at the surface of the sphere. When the diameter of the sphere becomes comparable with the mean free path of the molecules of the fluid, we have what corresponds to slip at the surface.

Kundt and Warburg¹ have given the following expression for the retarding force under these conditions:

$$F = F_{\eta} \left(1 - 0.71 \frac{l}{r} \right)$$

where F_{η} = retarding force in dynes due to viscosity; l = length of mean free path of gas particle in centimeters; r = radius of the sphere in centimeters.

McKeehan² has given an expression of the form

$$F = \frac{F_{\eta}}{\left(1 + A \frac{l}{r} \right)}$$

where A was calculated from theoretical assumptions to be equal to 1.05, following a method due to Cunningham, but making some corrections. His experiments gave A the value 1.00 ± 0.003 .

Stock³ has shown mathematically that a sphere falling freely near a plane boundary, but at a distance large compared to the radius of the sphere, is retarded slightly; the retarding force being expressed as follows:

$$F = F_{\eta} \left[\frac{1}{\left(1 - \frac{9}{16} \frac{r}{b} \right)} - \frac{r}{2b} \left(1 + \frac{9}{16} \frac{r}{b} \right) \right]$$

where F_{η} = retarding force in dynes due to viscosity; r = radius of the sphere in centimeters; b = distance between the sphere and boundary in centimeters.

¹ Poggendorf's *Annalen*, Vol. 155, p. 337 (1875).

² *Physical Review*, Vol. 33, p. 153 (1911).

³ *Bulletin de la Académie des Sciences de Cracovie*, Vol. 1 A, p. 18 (1911).

Stock also showed that there was no appreciable force tending to drive the sphere away from the boundary and that there was no tendency for the sphere to rotate.

For transparent fluids this method of measurement has many advantages. Visual observation can be employed in measuring the velocity, corrections for variations in densities of the fluids used can be kept small by using for the spheres materials that have a high density, and measurements can be made with the fluids subjected to mechanical pressure.

The serious drawbacks are the necessity of using very small spheres for materials of low viscosity and the limited range of any one choice of size of sphere and size of containing vessel. At low velocities convection currents in the fluid might seriously affect the accuracy of the determination.

5. *Rolling of a Sphere Down a Slanted Tube.*—This arrangement which has apparently not been tried before seems to possess the following advantages:

- (a) Simplicity;
- (b) Convenient increase of driving force by increase of slope and consequent reduction of time required for observations of the more viscous materials;
- (c) The possibility of further reduction in time by the use of spheres of greater density for the more viscous materials;
- (d) The possibility of still further reducing the time by decreasing the distance between transit points;
- (e) Visual observation is possible with transparent tubes even with black oils, since the sphere rolls along the bottom of the tube;
- (f) Measurements under mechanical pressure can easily be carried out;
- (g) Comparative ease of temperature control;
- (h) Small quantity of the sample used for a test;
- (i) Protection from evaporation;
- (j) Protection from exposure to room temperature.

The disadvantages are the absence of any information in regard to the laws of rolling motion along a boundary in a viscous medium, and the consequent lack of knowledge of the proportions suitable for experimental work. There is also the

disadvantage that corrections for variations in the specific gravity of the sample, while small (about 3 per cent with steel spheres and liquids between 0.8 and 1.2, sp. gr.), are not entirely eliminated. The effects of this variation are shown in Table III and Fig. 10.

In Fig. 2 is given a curve representing the results of a test made during the course of the preliminary work, to determine whether a sphere rolling down a slanted tube filled with distilled water obeys a law for its time of roll that corresponds at all with the viscosity of the medium. The result of this test was to show a fair agreement between time of roll and viscosity.

6. *Rotation of One of Two Concentric Spheres.*—This has been employed for gases by Zemplen¹ and seems to offer an opportunity for the construction of a simple type of instrument, from whose dimensions the viscous resistance could be calculated. It would be free from many of the disadvantages of the rotary drum or disk employed by Couette,² Drew,³ Searle,⁴ Maxwell,⁵ Meyer⁶ and others.

It is proposed to construct and test such an instrument as a continuation of the work presented here.

CHARACTERISTICS OF THE RESISTANCE OFFERED TO A SPHERE ROLLING DOWN A SLANTED TUBE THAT IS FILLED WITH A VISCOUS LIQUID.

No very high accuracy was attempted in the early tests which were carried out with fairly large spheres, only slightly smaller than the tubes. These tests were intended principally to give some idea of the relation of resistance to the ratio of diameter of sphere to diameter of tube.

A curve for one set of observations at small slopes is shown in Fig. 3, which is typical of a large number taken with water, indicating a velocity increasing at first directly as the driving force, that is, as the sine of the angle which the tube makes with

¹ *Annalen der Physik*, Vol. 29, p. 867 (1909); Vol. 38, p. 71 (1912).

² *Journal de Physique*, Vol. 9, p. 414 (1890).

³ *Physical Review*, Vol. 12, p. 114 (1901).

⁴ *Proceedings*, Cambridge Philosophical Soc., Vol. 16, p. 600 (1912).

⁵ *Philosophical Transactions*, Royal Soc., Vol. 144 p. 249 (1866).

⁶ *Poggendorf's Annalen*, Vol. 113, pp. 55, 113, 383 (1861); Vol. 125, p. 177 (1865); Vol. 143, p. 14 (1871); *Crelles' Journal*, Vol. 59, p. 282 (1861).

the horizontal. For the higher slopes, the velocity increases less rapidly, since the velocity soon reaches a value such that the inertia of the water displaced begins to offer a retarding force that is appreciable, compared to the viscous resistance.

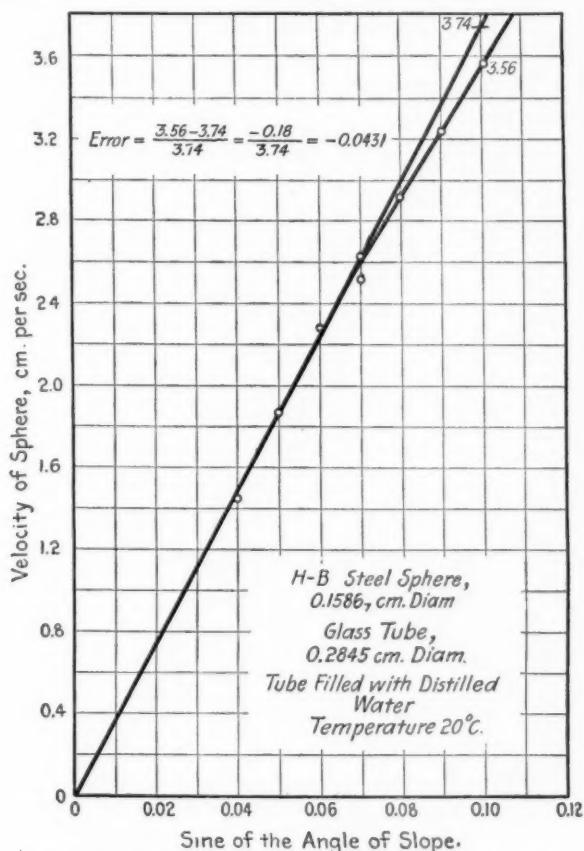


FIG. 3.—Relation of Speed and Driving Force of a Sphere Rolling down a Slanted Tube.

There was difficulty in all of these early tests at small slopes in getting the sphere to roll uniformly, the difficulty being either due to roughness of tube or irregularity of shape of the sphere. This difficulty disappears when higher slopes are used and when higher velocities are attained.

The relation of the rolling resistance to ratio of diameter of sphere to diameter of tube is illustrated by the curve shown in Fig. 4, which shows the marked and sudden increase in resistance for ratios exceeding 0.75, and led to a choice of the ratio of 0.5 for further work. At the lower ratio of diameters

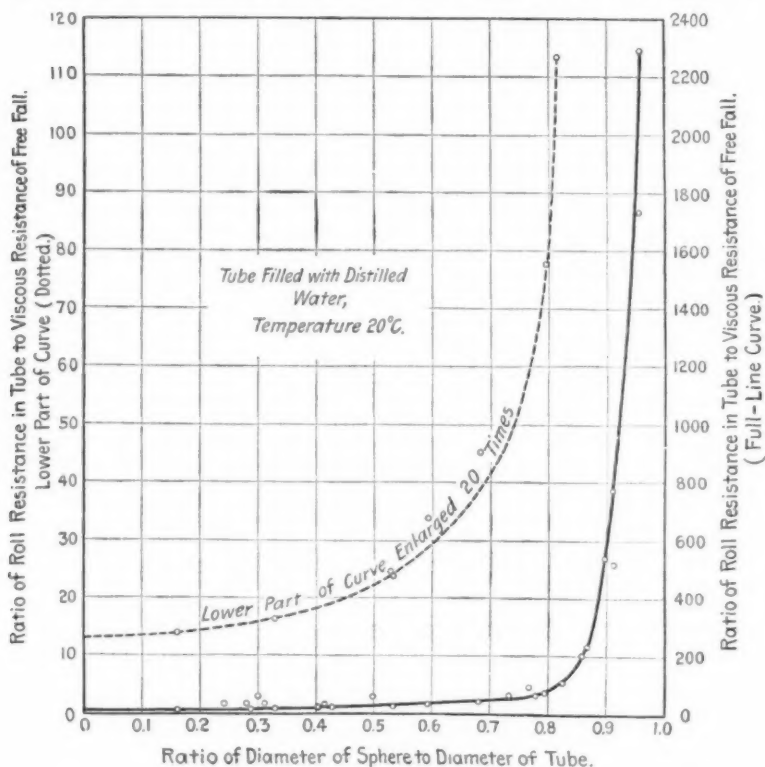


FIG. 4.—Effect of Diameter of Tube on Roll Resistance of Sphere.

small variations of size would produce little difference in the rolling resistance.

The effect of expansion due to rise of temperature on the time of roll may be determined as follows:

Let the velocity of free fall be

$$V = \frac{1}{18} \frac{d_s^2 g}{\eta} (\gamma_s - \gamma_m)$$

then the transit time in seconds per centimeter is

$$t = \frac{18 \eta}{d_s^2 g (\gamma_s - \gamma_m)}$$

where η = absolute viscosity in dynes; d_s = diameter of sphere in centimeters; g = force of gravity in dynes; γ_s = density of sphere in grams per cubic centimeter; γ_m = density of medium in grams per cubic centimeter.

Let the ratio of the resistance to motion in the tube to that of the free fall be R , as shown in the curve in Fig. 4. The transit time in the tube would then be:

$$t = R \frac{18 \eta}{d_s^2 g (\gamma_s - \gamma_m)}$$

$$t = \frac{R 18 \eta}{d_s^2 g \gamma_s \left(1 - \frac{\gamma_m}{\gamma_s}\right)}$$

The weight of the sphere is

$$W = \frac{\pi}{6} \gamma_s d_s^3 \quad \text{or} \quad \gamma_s = \frac{6W}{\pi d_s^3}$$

Substituting this for γ_s gives

$$t = \frac{R 18 \eta}{d_s^2 g \frac{6W}{\pi d_s^3} \left(1 - \frac{\gamma_m}{\gamma_s}\right)} = \frac{3\pi \eta R d_s}{W g \left(1 - \frac{\gamma_m}{\gamma_s}\right)} = \frac{3\pi \eta}{W g} \cdot \frac{R d_s}{\left(1 - \frac{\gamma_m}{\gamma_s}\right)}$$

Separating the parts,

$$t = \frac{3\pi \eta}{W g} \left(\frac{1}{1 - \frac{\gamma_m}{\gamma_s}} \right) (d_s) (R)$$

The effects of the expansion due to temperature rise may be investigated separately for the different parts of this expression.

Let β = expansion coefficient of the sphere material; τ =

expansion coefficient of the tube material; and in order to give a concrete idea of the possible magnitude of the correction let

$$\begin{aligned}\beta &= 14.0 \times 10^{-6} \text{ for hard steel;} \\ \tau &= 8.6 \times 10^{-6} \text{ for glass;} \\ \theta &= 300^\circ \text{ C.;} \\ \beta\theta &= 4.2 \times 10^{-3}; \\ \tau\theta &= 2.58 \times 10^{-3} \approx 2.6 \times 10^{-3} \\ \gamma_s &= 7.757; \\ \gamma_m &= 1.\end{aligned}$$

Then writing

$$t = K \frac{1}{1 - \frac{\gamma_m}{\gamma_s}}$$

and putting

$$\begin{aligned}\gamma_s &= \frac{6W}{\pi d_s^3} \\ t &= K \frac{1}{1 - \frac{\gamma_m \pi d_s^3}{6W}}\end{aligned}$$

Using the subscripts 0 and θ to indicate the values at the reference temperature and the temperature θ respectively the ratio error due to change of density may be expressed as

$$\begin{aligned}E_\gamma = \frac{t_\theta - t_0}{t_0} &= \frac{\frac{1}{1 - \frac{\gamma_m \pi d_{s\theta}^3 (1 + \beta\theta)^3}{6W}} - \frac{1}{1 - \frac{\gamma_m \pi d_{s0}^3}{6W}}}{\frac{1}{1 - \frac{\gamma_m \pi d_{s0}^3}{6W}}} \\ E_\gamma &= \frac{1 - \frac{\gamma_m \pi d_{s0}^3}{6W}}{1 - \frac{\gamma_m \pi d_{s\theta}^3 (1 + \beta\theta)^3}{6W}} - 1 = \frac{1 - \frac{\gamma_m}{\gamma_s}}{1 - \frac{\gamma_m}{\gamma_s} (1 + \beta\theta)^3}\end{aligned}$$

Putting $(1 + \beta\theta)^3 = (1 + 3\beta\theta)$

$$E_\gamma = \frac{1 - \frac{\gamma_m}{\gamma_s} - 1 + \frac{\gamma_m}{\gamma_s}(1 + 3\beta\theta)}{1 - \frac{\gamma_m}{\gamma_s}(1 + 3\beta\theta)} = \frac{+ 3\beta\theta \frac{\gamma_m}{\gamma_s}}{1 - \frac{\gamma_m}{\gamma_s}(1 + 3\beta\theta)}$$

For the particular values assumed above,

$$\begin{aligned} E_\gamma &= \frac{3 \times 4.2 \times 10^{-3} \frac{1}{7.757}}{1 - \frac{1}{7.757}(1 + 3 \times 4.2 \times 10^{-3})} = \\ &= \frac{1.625 \times 10^{-3}}{1 - \frac{1}{7.757}(1 + 12.6 \times 10^{-3})} = \frac{1.625 \times 10^{-3}}{1 - \frac{1}{7.757}(1 + 0.0126)} = \\ &= \frac{1.625 \times 10^{-3}}{1 - 0.1308} = \frac{1.625 \times 10^{-3}}{0.87} \\ E_\gamma &= 1.868 \times 10^{-3} \approx 0.0019 \end{aligned}$$

The equation may also be put in the form,

$$t = K d_s$$

The error due to expansion of the sphere is

$$\begin{aligned} E_s &= \frac{t_\theta - t_0}{t_0} = \frac{K d_{s\theta} - K d_{s0}}{K d_{s0}} \\ E_s &= \frac{K d_{s0}(1 + \beta\theta) - K d_{s0}}{K d_{s0}} = \frac{(1 + \beta\theta) - 1}{1} \\ E_s &= \beta\theta \end{aligned}$$

For the particular case of a steel sphere and 300° C. rise,

$$E_s = 4.2 \times 10^{-3} = 0.0042$$

The equation may also be put in the form,

$$t = K R$$

Now R is the ratio of the resistance in the tube to that of free fall. At any point where the resistance is R_1 the change due to temperature gives some new value,

$$R_\theta = R_1 + m \Delta \left(\frac{d_s}{d_r} \right)$$

where m = the slope of the tangent at R_1 ; d_s = diameter of sphere; d_r = diameter of tube bore; $\left(\frac{d_s}{d_r} \right)$ = the increment of ratio of size due to temperature.

The error may be written

$$E_R = \frac{t_\theta - t_0}{t_0} = \frac{K R_\theta - K R_1}{K R_1} = \frac{R_\theta - R_1}{R_1} = \frac{R_1 + m \Delta \left(\frac{d_s}{d_r} \right) - R_1}{R_1}$$

$$E_R = \frac{m}{R_1} \Delta \left(\frac{d_s}{d_r} \right)$$

$$E_R = \frac{m}{R_1} \left[\frac{d_s (1 + \beta \theta)}{d_r (1 + \tau \theta)} - \frac{d_s}{d_r} \right]$$

$$E_R = \frac{m}{R_1} \frac{d_s}{d_r} \left[\frac{1 + \beta \theta}{1 + \tau \theta} - 1 \right] = \frac{m}{R_1} \frac{d_s}{d_r} \left[\frac{1 + \beta \theta - 1 - \tau \theta}{1 + \tau \theta} \right]$$

neglecting $\tau \theta$ with regard to $1 + \tau \theta$

$$E_R = \frac{m}{R_1} \frac{d_s}{d_r} \left(\frac{\beta \theta - \tau \theta}{1} \right)$$

The particular values chosen for illustration give at 300° C. rise

$$E_R = \frac{m}{R_1} \frac{d_s}{d_r} (4.2 \times 10^{-3} - 2.6 \times 10^{-3})$$

$$E_R = \frac{m}{R_1} \frac{d_s}{d_r} 1.6 \times 10^{-3}$$

From the test curve in Fig. 4 for R at $\frac{d_s}{d_r} = 0.5$, $m = 36$,
 $R_1 = 22.7$.

$$E_R = \frac{36}{22.7} 0.5 \times 1.6 \times 10^{-3} = 1.268 \times 10^{-3}$$

$$E_R \approx 0.0013$$

At $\frac{d_s}{d_r} = 0.6$, $m = 87.7$, $R_1 = 29.8$.

$$E_R = \frac{87.7}{29.8} 0.6 \times 1.6 \times 10^{-3} = 2.83 \times 10^{-3}$$

At $\frac{d_s}{d_r} = 0.7$, $m = 160.4$, $R_1 = 41.7$

$$E_R = \frac{160.4}{41.7} 0.7 \times 1.6 \times 10^{-3} = 4.31 \times 10^{-3} = 0.00431$$

At $\frac{d_s}{d_r} = 0.75$, $m = 305$, $R_1 = 53.1$

$$E_R = \frac{305}{53.1} 0.75 \times 1.6 \times 10^{-3} = 6.9 \times 10^{-3} = 0.0069$$

The expansion of the tube gives a greater distance for the sphere to roll between the transit marks and consequently a longer time of roll. The error due to this effect may be taken as

$$E_r = \frac{t_\theta - t_0}{t_0} = \frac{L_\theta - L_0}{L_0} = \frac{L_0 (1 + \tau\theta) - L_0}{L_0}$$

$$E_r = \tau\theta$$

For the particular case chosen at 300°C. ,

$$E_r = 2.6 \times 10^{-3}$$

If the total error due to temperature be taken as the sum of the part errors then,

$$E_{\theta} = E_{\gamma} + E_s + E_r + E_R$$

$$= 0.0019 + 0.0042 + 0.0026 + \left\{ \begin{array}{l} 0.0013 \text{ at } \frac{d_s}{d_r} = 0.5 \\ 0.0028 \text{ at } \frac{d_s}{d_r} = 0.6 \\ 0.0043 \text{ at } \frac{d_s}{d_r} = 0.7 \\ 0.0069 \text{ at } \frac{d_s}{d_r} = 0.75 \end{array} \right.$$

$$E_{\theta} = \left\{ \begin{array}{l} 0.0100 \text{ at } \frac{d_s}{d_r} = 0.5 \\ 0.0115 \text{ at } \frac{d_s}{d_r} = 0.6 \\ 0.0130 \text{ at } \frac{d_s}{d_r} = 0.7 \\ 0.0156 \text{ at } \frac{d_s}{d_r} = 0.75 \end{array} \right.$$

This shows that the total error even for 300° C. rise is of the order of 1 to 1.5 per cent, and is consequently about the same as that of a capillary-tube measurement using a copper or steel outflow tube.

The expansion of the sphere with temperature rise is the preponderating cause in change of time of roll, consequently a sphere of material having a small temperature coefficient of expansion should be chosen if possible.

The intersection of the curve plotted between ratio of roll resistance to free-fall resistance, and the ratio of sphere diameter to tube diameter, gives on the axis of ordinates (ratio of resistances) a value of about 12. This value may be interpreted as the increase of resistance to the motion of a sphere in a viscous medium of infinite extent when forced to roll along a plane boundary.

Timing the motion between marks 6 cm. apart, instead of

30 cm., was tried with the more viscous materials, as may be seen from Table II. Sets of observations using this roll distance could be repeated with an accuracy of 0.2 per cent, and were within 0.5 per cent of the time calculated as one-fifth of the time required for 30 cm. This difference may be due to slight variations in the tube diameter, to nearness of the transit points to the end of the tube, or to errors of at least 0.03 cm. in locating the transit points. It would be perfectly

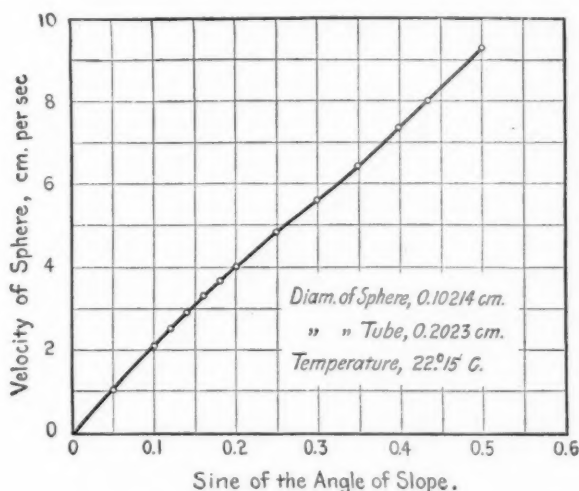


FIG. 5.—Effect of Slope on Velocity in Distilled Water; Sphere-and-Tube Viscosimeter.

easy to calibrate and set marks for one-tenth the time taken to roll the longest distance.

It is to be expected, if the resistance varies according to the general law for fluids, that the resistance (which is equal to the driving force after the maximum velocity is attained) would give a velocity increasing directly as the driving force at first, when V^2 is small compared to V . When curves connecting velocity and slope (or the driving force) are plotted we should then find that they would be straight lines at first with a droop in the upper part of the curve. One such curve is shown in Fig. 3 to illustrate the limits of velocity that could be used for one selection of sphere and tube, using water as the

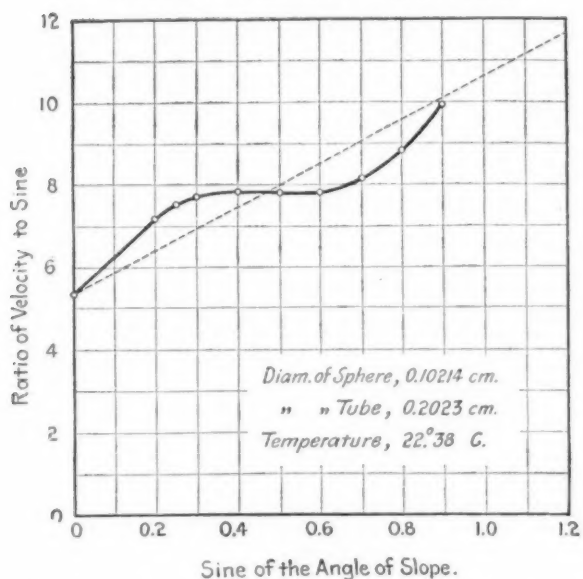


FIG. 6.—Effect of Slope on Velocity in 34.4 per cent of Glycerin; Sphere-and-Tube Viscosimeter.

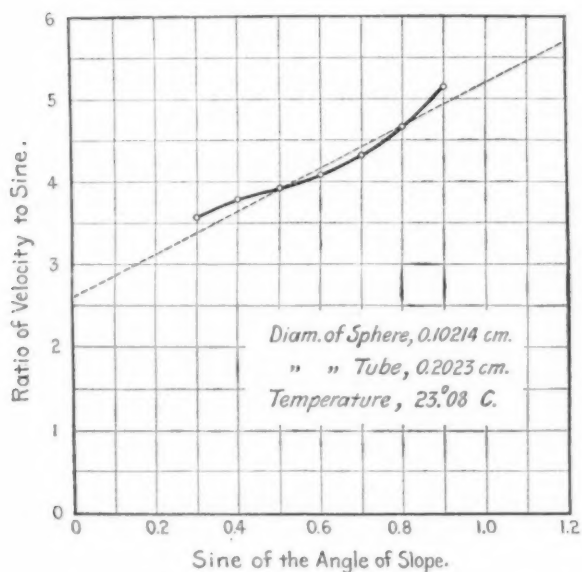


FIG. 7.—Effect of Slope on Velocity in 51 per cent of Glycerin; Sphere-and-Tube Viscosimeter.

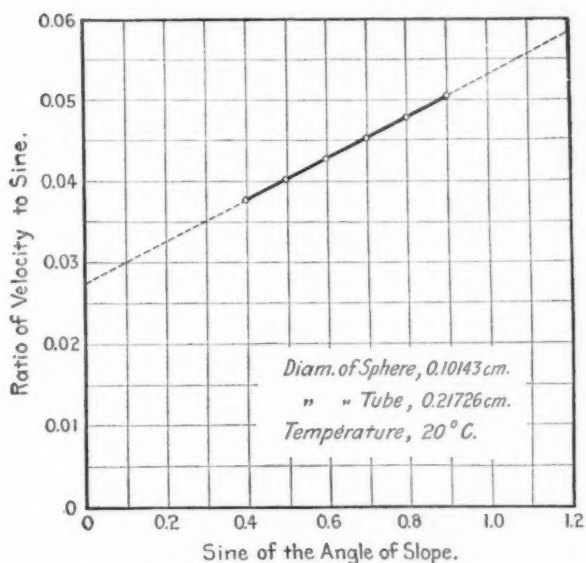


FIG. 8.—Effect of Slope on Velocity in 100 per cent of Glycerin; Sphere-and-Tube Viscosimeter.

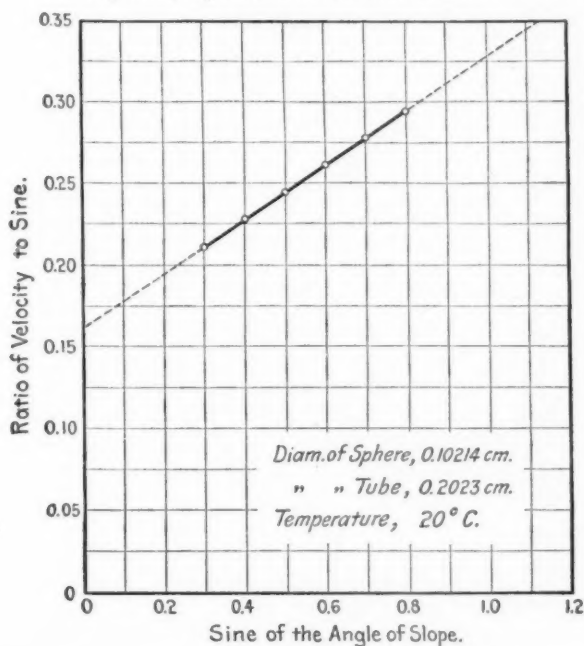


FIG. 9.—Effect of Slope on Velocity in Oil No. 6; Sphere-and-Tube Viscosimeter.

viscous medium. A second curve, Fig. 5, with smaller sphere and tube, shows a much higher slope before the inertia effects begin to appear and so limit the increase of velocity.

For very high slopes the form of motion appears to be considerably modified. If a liquid is used of viscosity at least five times that of water, so that at no time do the inertia effects appear, we find the velocity increasing more rapidly than the slope. These effects are illustrated in Figs. 6, 7, 8 and 9, and Table I. The curves have been plotted between the ratios of velocity to the sine of the angle of slope as ordinates, and the

TABLE I.—EFFECT OF SLOPE ON VELOCITY OF SPHERE.
AVERAGE OF 6 CURVES.

$\sin \alpha$.	Ratio of $\frac{V}{\sin \alpha}$ to intercept where $\sin \alpha = 0$.
0.0	1.000
0.1	1.101
0.2	1.201
0.3	1.302
0.4	1.403
0.5	1.504
0.6	1.603
0.7	1.704
0.8	1.807
0.9	1.907
1.0	2.012

Interval covered
by measurements

sines of the angles of slope as abscissas ($\sin \alpha$), for tests where the sphere had a diameter one-half that of the tube.

In the six tests averaged above the liquids used were as follows:

Fifty-six per cent of glycerin; 80 per cent of glycerin; oil No. 6; 100 per cent of glycerin; and the viscosities varied from about 15 to about 800 times that of water at 20° C.

Although the intersections at $\sin \alpha = 0$ and $\sin \alpha = 1$ had to be obtained by extension of the curves, the intersection ratio was in no case less than 1.94 or greater than 2.07. These variations are within the limits of experimental error in making the determinations and in using the plotted results. The

velocity thus is shown to increase more rapidly than the driving force but at the same rate throughout this wide range of viscosities.

It is also to be noted that the relation between any pair of points on these curves is

$$\frac{\frac{V_2}{\sin \alpha_2}}{(1 + \sin \alpha_2)} = \frac{\frac{V_1}{\sin \alpha_1}}{(1 + \sin \alpha_1)}$$

$$\frac{V_2}{\sin \alpha_2(1 + \sin \alpha_2)} = \frac{V_1}{\sin \alpha_1(1 + \sin \alpha_1)}$$

$$V_2 = \frac{\sin \alpha_2(1 + \sin \alpha_2)}{\sin \alpha_1(1 + \sin \alpha_1)} V_1$$

The corresponding transit times t are

$$t_2 = \frac{\sin \alpha_1(1 + \sin \alpha_1)}{\sin \alpha_2(1 + \sin \alpha_2)} t_1$$

At any given slope, however, the time of roll increases directly as the viscosity. In Table II is given the summary of a number of comparative measurements of viscosity. One series of measurements was made with a capillary tube as a standard method of comparison. The other series of measurements was made with a 1-mm. steel sphere and a 2-mm. glass tube.¹ This comparison was extended far beyond the range of viscosity of lubricating oils, and was even extended to water. In the case of water, however, a slope of 0.132 was used. This slope was chosen because it should give just one-fifth the velocity of a slope of 0.5, assuming that the effect of slope on velocity is the same as for the more viscous materials. Even with this handicap the comparison in the case of water and the materials of low viscosity is very satisfactory.

The correcting factor due to variations of specific gravity to be applied to the time readings, to obtain the true relative viscosities, may be expressed as follows:

$$K_v = \frac{\gamma_s - \gamma_m}{\gamma_s - 1},$$

¹ This sphere, a standard ball-bearing ball, was supplied through the kindness of Mr. J. E. Diamond, of the Hess-Bright Manufacturing Co.

TABLE II.—SUMMARY OF COMPARATIVE TESTS OF VISCOSITY.

Liquid Tested and Date of Test.	CAPILLARY-TUBE METHOD.						SPHERE-AND-TUBE METHOD.								Date of Test.	
	Temperature, deg. Cent.	Specific Gravity, g. per cc.	Time of Outflow, sec.	Average Head, cm.	Quantity, cc.	Relative Viscosity, dynes (η_g).	Absolute Viscosity, dynes (η).	Sine of Angle, Slope.	Roll Distance used in Timing, cm.	Roll Time, sec.	Roll Time reduced to 30 cm. at $\sin \alpha, 0.5$ sec.	Relative Roll Time, sec.	Effective Specific Gravity (K_γ).	Relative Roll Time reduced to Sp. Gr. = 1, or Relative Viscosity by Sphere-and-Tube Method = η_g .		Sphere-and-Tube Constant, K.
Water, distilled, filtered through Porous Porcelain, May 24, 1912	20.0	0.9982	177.6	62.5	5.0	1.000	0.01006 ^a 0.01076 $\eta = 0.01006 \eta_g$	0.132	30	10.9	2.18	1.000	1.00015	1.0001	0.00461	May 26, 1912.
Water and 34.4 per cent Glycerin, May 25, 1912.	20.0	1.137	572.0 ^c 230.8 ^d	62.5 64.0	5.0 2.0	3.55 3.66	0.0357 0.0368	0.5	30	8.31	8.31	3.815	0.986	3.76	0.00449	May 27, 1912.
Water and 51 per cent Glycerin, May 28, 1912.	20.0	1.151	236.7	64.5	1.0	7.82	0.0786	0.5	30	17.37 17.51	17.37 17.51	7.97 8.03	0.980 0.980	7.81 7.87	0.00462 0.00446	May 28, 1912. May 29, 1912.
Engine Oil, No. 6, April 25, 1912.	20.0	0.808	3915.0	67.05	1.7	114.4	1.151	0.5	30	247.4	247.4	113.4	1.020	115.7	0.00457	April 20-25, 1912.
Glycerin, May 7 and June 13, 1912	16.65	1.2585	144,000 (40 hrs.)	3.5	1362.0	15.70	0.5	6	572	2860.0	1312.0	0.962	1262.0	0.00498	June 13-15, 1912.

 η = Density or sp. gr. of material in g. per cc. h = Head in cm. g = Force of gravity in dynes. d = Diameter of capillary in cm. t = Time of outflow in sec. Q = Quantity of liquid in cc. l = Length of capillary in cm.^a As given by Hosking.^c Sample No. 1. $\eta = K K_\gamma t_{30}^{1/2}$ $K_\gamma = \frac{\gamma_s - \gamma_m}{\gamma_g - 1}$ γ_s = Density of sphere in grams per cubic centimeter. γ_m = Density of material tested. $t_{30}^{1/2}$ = Time to roll 30 cm. on slope whose sine is = 0.5.^b As calculated from dimensions of capillary.^d Sample No. 2. This sample was used in sphere-and-tube test.

where γ_s = density of the sphere material in grams per cubic centimeter; γ_m = density of the material tested in grams per cubic centimeter; 1 = reference standard density.

The absolute viscosity may be calculated from the time readings by means of the following expression:

$$\eta = K K_\gamma t$$

where η = absolute viscosity in dynes; K = constant depending on the diameters of sphere and tube, the slope, and the

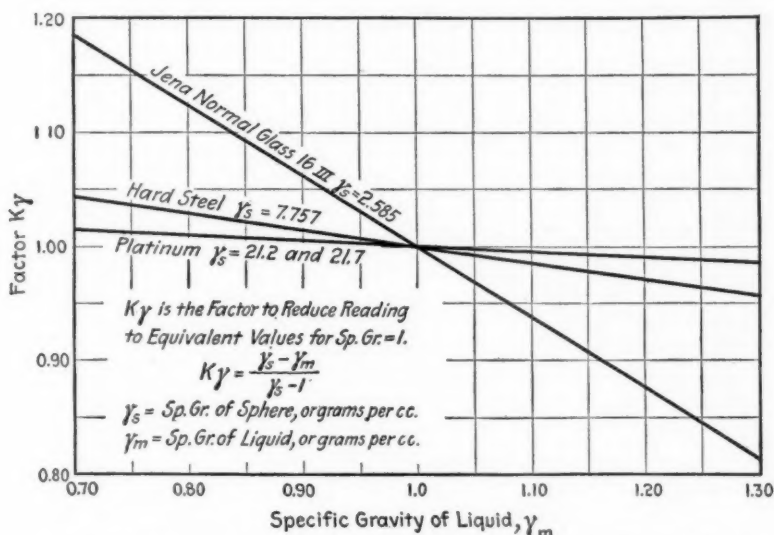


FIG. 10.—Effect of Specific Gravity on Net Driving Force and Roll Time for an Immersed Sphere.

roll distance; K_γ = the correcting factor due to the density of the material tested; t = time of roll in seconds.

Table III gives the values of the correcting factor due to the density of the material tested for several materials. These results are plotted in Fig. 10.

Fig. 11 gives an outline of an apparatus embodying the principles of the new viscosimeter in which the time required for a sphere to roll down a slanted tube filled with a liquid is taken as a measure of the viscosity of that liquid. At (a) is

TABLE III.—EFFECT OF SPECIFIC GRAVITY OF A LIQUID ON THE NET DRIVING FORCE AND ROLLING TIME OF AN IMMERSSED SPHERE.

Sphere, Material.	Specific Gravity of		Difference of Specific Gravities.	Factor to Reduce Readings to Standard Sp. Gr.=1, K_{γ} .
	Sphere.	Liquid.		
Jena Normal Glass 16, III.....	2.585	1.3	1.285	0.811
		1.2	1.385	0.874
		1.1	1.485	0.937
		1.0	1.585	1.000
		0.9	1.685	1.063
		0.8	1.785	1.126
		0.7	1.885	1.185
Hard Steel.....	7.757	1.3	6.457	0.955
		1.2	6.557	0.970
		1.1	6.657	0.985
		1.0	6.757	1.000
		0.9	6.857	1.014
		0.8	6.957	1.030
		0.7	7.057	1.044
Platinum.....	21.2	1.3	19.9	0.985
		1.2	20.0	0.990
		1.1	20.1	0.995
		1.0	20.2	1.000
		0.9	20.3	1.005
		0.8	20.4	1.010
		0.7	20.5	1.015
Platinum.....	21.7	1.3	20.4	0.985
		1.2	20.5	0.990
		1.1	20.6	0.995
		1.0	20.7	1.000
		0.9	20.8	1.005
		0.8	20.9	1.010
		0.7	21.0	1.015

NOTE.— $K_{\gamma} = \frac{\gamma_s - \gamma_m}{\gamma_s - 1}$ where γ_s = specific gravity of the sphere;

γ_m = specific gravity of the liquid.

shown a tube which may be filled with the liquid and closed by ground glass stoppers. At (b) is shown a general assembly with the tube for testing the viscosity placed inside a larger outer tube which serves as an oven. The temperature may be changed either by heated air or some transparent liquid, or by electrically heated wires.

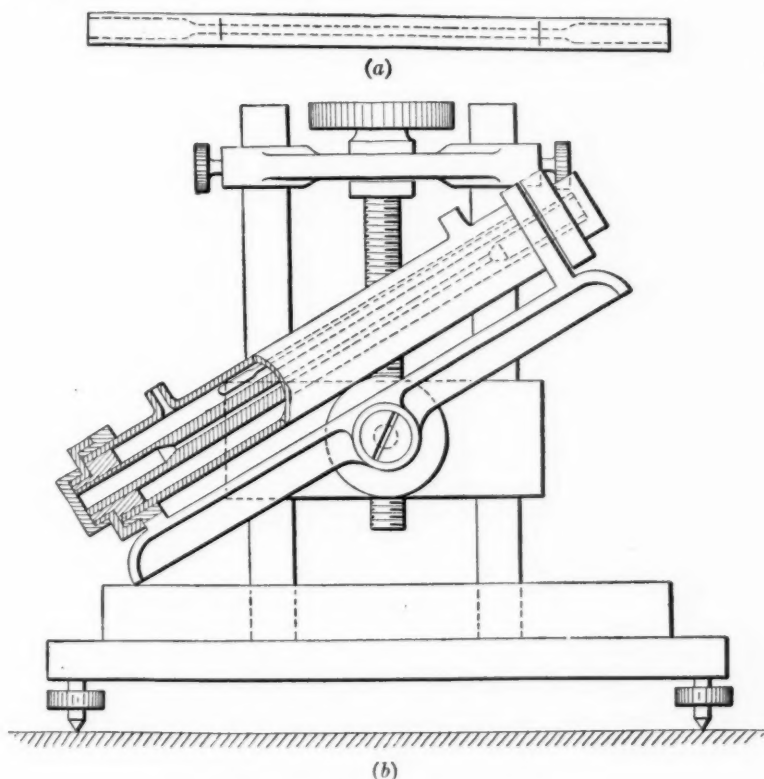


FIG. 11.—The New Viscosimeter.

The whole subject of the effect of temperature upon viscosity needs further investigation. There are strong indications that both the length of exposure to any temperature and the direction of change of temperature affect the viscosity. For these reasons, there would be many advantages in the use of a large thermostatically controlled oven or box, where the visco-

simeter and quantities of the samples would be kept and repeatedly tested under definite conditions as to temperature.

It is a pleasure to acknowledge the assistance given to the carrying out of this work by the University of Missouri, Cornell University, and the Telluride Association.

SUMMARY.

The results may be summarized as follows:

1. In general, fluid resistance must be expressed by at least two terms, as

$$R = (K_1 s) V + (K_2 \gamma) V^2$$

where K_1 and K_2 are arbitrary coefficients; s = surface area; γ = specific gravity of the fluid; V = velocity.

2. The term containing the first power of the velocity V gives the so-called viscous resistance, and in general, in order to have the motion controlled by the viscous resistance, the term containing V^2 must be negligible compared to that containing V .

3. The critical velocity of flow in tubes as determined by Reynolds does not apply to the very short outflow tubes used in the commercial flow types of viscosimeters.

4. The errors caused by exceeding the critical velocity for materials of low or moderate viscosity, the transition at some indeterminate point to conditions for viscous flow, and the energy required for accelerating the fluid to the outflow velocity, are large and variable in the commercial flow types of viscosimeters. The time required for making observations with viscous materials, and the difficulties of temperature control and measurement, are additional disadvantages.

5. A number of different methods of viscosity measurement have been discussed, and results that may be obtained with the sphere rolling down a slanted tube described.

CONCLUSIONS.

The conclusions that may be drawn from this work are:

1. The use of present flow-type viscosimeters should be discontinued.

2. The sphere-and-tube type of viscosimeter may be used for comparisons of absolute viscosity over a wide range with a moderate error.

3. Some of the advantages of the sphere-and-tube type of viscosimeter are:

- (a) Small amount of sample needed;
- (b) Protection from evaporation;
- (c) Protection from exposure to room temperature between check readings;
- (d) Short length of time needed for an observation;
- (e) Convenient and quickly made check readings by reversal of slope;
- (f) Smallness of correction for variation of specific gravity of sample;
- (g) Possibility of adjustment of observation time for very viscous materials by the use of denser spheres, higher slopes or a shorter roll distance (the reduction of roll distance is the most convenient and accurate method to employ);
- (h) Tests could be made at high temperature and under pressure;
- (i) The sphere diameter should be from one-half to three-quarters the diameter of the tube.

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RUBBER BELTING AND METHODS OF TESTING.

BY W. E. CAMPBELL.

SUMMARY.

Power Belting.—For transmission of power.

Conveyor Belting.—For conveyance of materials. It is pointed out that rubber, because of its resilient properties, resists abrasion well and hence makes it the best conveying surface.

Elevator Belting.—A special form of conveyor belting for grain elevators.

Cotton Duck.—The weight, strength and construction are generally specified. The advisability of putting greater strength in the warp than in the filler is pointed out.

Rubber Friction Stock.—So called because of the method used in its manufacture.

Rubber Cover Stock.—Specifications for this generally include such physical properties as tensile strength, ultimate elongation, and permanent set. Belts having no cover are said to be "friction covered."

Rubber Beading Stock.—This fills in the splice in the duck in the outer ply.

Friction Test.—Determination of adhesion between plies of duck. This can best be determined on a dynamometer which registers the force (in pounds) necessary to separate two contiguous plies, at the rate of 1 in. stripped per minute, on a test specimen 1 in. wide.

Physical Properties of Cover Stock.—This comprises (1) determination of adhesion to duck underneath, (2) determination of tensile strength and ultimate elongation, (3) determination of permanent set, which is a measure of the degree of vulcanization.

Physical Properties of Plies of Duck.—Determination of tensile strength and stretch of the plies of the belt. The amount of stretch remaining in the belt is determined on the whole section of the belt.

RUBBER BELTING AND METHODS OF TESTING.

By W. E. CAMPBELL.

GENERAL CLASSES OF RUBBER BELTING.

Power Belting.—Belting for the transmission of power is the commonest class of belting and is manufactured with, and in some cases without, thin rubber covers. It includes a wide range of qualities, from ordinary factory transmission belting where the work is not overly severe, to belting for paper mills and saw mills, where the work is very severe and excessive stretch in the belt cannot be tolerated.

A demand has also been created in recent years by certain railroad companies for a high grade of power belting for axle generators, and the conditions in this case are such as to require a special light-weight and flexible belt of great strength, and having all or nearly all the stretch removed during the manufacturing processes.

Conveyor Belting.—This class of belting is used for conveying ore, coal, sand, gravel, marl, slag, etc. The rubber edges and "face" cover are made very thick, in order to withstand the heavy abrasive action and severe use to which a belt in this class of service is subjected. Rubber, because of its peculiar resilient properties, holds a unique place as a conveying surface in this class of work, resisting wear when properly manufactured much better than any other known material.

Some conveyor belts have the face cover thickest along the middle of the belt, becoming thinner outward toward the edges; under certain other conditions, however, such as when the material to be conveyed has to be taken up out of water (for instance, conveying hot smelter slag, cooled down by water, to the dump) the back cover as well as the face cover is of thick construction.

Elevator Belting.—These are really conveyor belts for use in grain elevators. They are manufactured without the thickened covers, the ordinary thickness of cover being all that is necessary for this work.

Since conveyor belting uses up power instead of transmitting it, its efficiency depends upon such features as (1) lightness in weight consistent with adequate strength, (2) flexibility in order to trough well, and (3) the ability to run straight and true.

MATERIALS OF CONSTRUCTION.

The materials which enter into the construction of rubber belting may be classified as follows: (1) cotton duck, (2) rubber friction stock, (3) rubber cover stock, and (4) rubber beading stock.

Cotton Duck.—Belting ducks range in weight from 22 to 32 oz. per linear yard, 42 in. wide, and are manufactured in 42 and 50-in. widths. Specifications for rubber belting generally state the weight of duck required, the minimum strength of both warp and filler, and sometimes the construction of warp and filler (specifying the number of yarns per inch each way, and the number of plies in each).

Since the warp yarns run lengthwise of the belt they must necessarily carry the load, and it is quite apparent that the warp should be constructed with a tensile strength much greater than that of the filler. It is not uncommon, however, to come across a series of belting ducks, 26, 28, and 30-oz. weights, of which the warp in the 28 and 30-oz. ducks is no stronger than the warp of the 26-oz. duck, the extra weight in each case having been put in the filler. This is obviously a great mistake.

Rubber Friction Stock.—This is so called, for the reason that while it is in an unvulcanized plastic condition, it is forced into the structure of the individual yarns and the interstices of the duck by means of a calender, through which it passes simultaneously with the duck between very large and heavy cast-iron rolls, revolving toward each other but with the roll which comes in contact with the rubber, revolving at a greater speed than the other. As a result of this difference in speed the "frictioning" effect is produced. When both sides of the duck have thus been frictioned, it is passed through twice with the rolls revolving at equal speed, which has the effect of pressing a coat or "skim" of rubber stock upon each side of the duck.

The adhesion between the plies of a belt is dependent upon (1) the quality of the friction stock, (2) the quantity of friction

stock on the duck, and (3) the degree of completeness with which the friction stock has been driven into the duck. It is obvious also that the construction of the duck has a bearing on this point, since the more tightly constructed fabrics are much more difficult to properly friction than the fabrics possessing a more open structure. Another point which has a bearing on the ultimate adhesion between the plies is (4) the manner in which the belt has been vulcanized under pressure in the hydraulic press.

Rubber Cover Stock.—Transmission belts which possess covers are, as a general rule, covered on both sides with the same rubber stock. Transmission belts which have no proper cover are said to be "friction" covered—that is, no extra cover has been applied to the belt other than that imparted to the duck by the frictioning process.

The thick face and back covers on conveyor belts may be composed of two rubber stocks of different quality; the face cover being made of a tougher rubber stock than is necessary for the opposite side, which is not called upon to carry the abrasive load.

Specifications on cover stock are generally designed to cover such points as tensile strength, ultimate elongation and permanent set.

Rubber Beading Stock.—This is generally a high-grade rubber stock which runs longitudinally and fills in the splice in the duck on the side of the belt opposite the pulley side. Specifications generally state that this beading stock must amalgamate well with the belt so that no parting is likely to occur between the beading and the ordinary cover.

METHODS OF TESTING.

Friction Test.—This is made by determining the adhesion between the plies of duck. The test can best be carried out on a dynamometer so designed that it may be adjusted to separate the plies at the rate of 1 in. stripped per minute, on a test specimen 1 in. wide cut from the belt, either longitudinally or transversely as desired. Such a dynamometer is illustrated in Fig. 1. Two jaws hold the sample, one being situated vertically above the other, the upper one connecting with a chart which records

the amount and also the duration of pull; the lower jaw is actuated by a motor which causes it to descend at a given speed.

To test any one ply, it is separated for a short distance at one end of the sample and the unseparated portion of this same end is fixed in the upper jaw and the separated ply is fixed in the lower descending jaw, so that the two ends are thus held

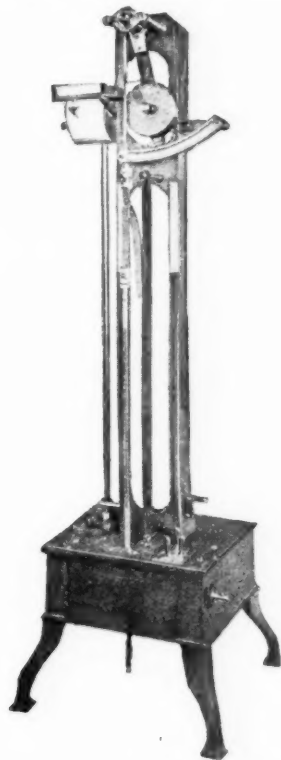


FIG. 1.—Dynamometer for Testing Rubber Belting.

in the same plane and separated in opposite directions at a rate which allows 1 in. (or 1 sq. in.) to be separated per minute.

Physical Properties of Cover Stock.—Adhesion of cover to the frictioned duck underneath can be determined in a manner exactly the same as described under "Friction Test."

Tensile strength and ultimate elongation are determined

as follows: The cover is carefully removed (if necessary, by a treatment with naphtha, afterwards drying carefully and thoroughly) and a longitudinal test specimen is then prepared by stamping out with a die. This gives a specimen $\frac{1}{2}$ in. by 2 in. long, with the ends beyond the 2 in. enlarged to a width of 1 in. where they fit into the clamps which hold the specimen in position during the tension test. By using a specimen so designed, all fear of its breaking in the clamps is removed.

A test specimen is marked with datum lines 2 in. apart, carefully gaged, and then adjusted in the dynamometer and stretched to the breaking point at the rate of 6 in. (or any specified rate) per minute. The breaking stress is recorded on the chart, and the ultimate elongation is determined by following the two datum lines along the fixed measure scale situated at the side of the dynamometer.

Permanent set determines whether the cover stock is adequately vulcanized. A longitudinal section of the cover stock 1 in. wide has marks placed upon it 2 in. apart. It is then placed in the dynamometer and stretched until the two lines are a specified distance apart, which distance depends upon the quality of the rubber stock. It is immediately released and re-marked as above, and again stretched until these latter two marks are the specified distance apart. The specimen is held there for 10 minutes. It is then released and 10 minutes afterwards the distance between the second pair of marks is measured. The excess over 2 in. is called the permanent set, and is a measure of the degree of vulcanization.

Physical Properties of Plies of Duck.—Specifications for belting which state the minimum allowable tensile strength of the duck should also specify the method to be used in testing the material, stating width of test specimen, distance between the jaws of the dynamometer, and the rate of separation of jaws in making the actual test. It is sometimes customary, when the sample of belting is very small, to use a specimen 3 in. wide, determining the tensile strength by the use of jaws 1 in. wide, placed 1 in. apart and directly opposite one another on the center of the test section, both jaws gripping the same longitudinal yarns. An alternative method sometimes employed consists in breaking several separate yarns and multiplying the average result by the

number of yarns per inch. The author has compared these two latter methods on the same material and found that the former method gave results as high as 29.2 per cent greater on the warp and 9.5 per cent greater on the filler.

The amount of stretch remaining in the belt may be determined by placing the whole width (or fraction of width) of belt in a tension testing machine, loading the belt up to or beyond the actual service load, and calculating the percentage of stretch. For example, a current specification for axle generator belting uses the full width of belt with 18 in. between jaws. Under a load equivalent to the product of the number of plies of duck multiplied by the width in inches, multiplied by 200, it must not elongate over $1\frac{1}{2}$ in. or 8.3 per cent.

DISCUSSION.

- The Chairman.** THE CHAIRMAN (PRESIDENT ARTHUR N. TALBOT).—The Chair would like to ask about this term, "friction test." I understand that we are really at the beginning of the standardization work of Committee D-11 on Standard Specifications for Rubber Products, and it would seem a good time to formulate terms carefully, and not perpetuate something which may be misleading. As I understand this term "friction test," it is not really a friction test, it is a stripping or separation test or shearing test, or, possibly, as the word has been used here once, an adhesion test, depending upon the way in which the test is made. Is not that true?
- Mr. Campbell.** MR. W. E. CAMPBELL.—The term "friction" is so called from the process employed in forcing the unvulcanized and plastic rubber stock into the interstices of the cotton duck previous to making up the belt. The duck and the rubber stock are passed simultaneously through a calender of which the upper roll revolves at a greater speed than the lower and hence forces or "frictions" the rubber into the duck. The friction test is therefore made by determining the adhesion between the plies of the finished belt.
- The Chairman.** THE CHAIRMAN.—Is it not a pulling apart?
- Mr. Campbell.** MR. CAMPBELL.—Yes, it is pulling apart.
- Mr. Rathjens.** MR. GEORGE RATHJENS.—Does this friction test apply only to those belts that are made of various plies of some particular material? If not, how would you apply this test where the stock is woven into one unit?
- Mr. Campbell.** MR. CAMPBELL.—Some rubber belting is reinforced by being stitched longitudinally. The method of making a friction or stripping test on stitched belts is the same as for unstitched belts, except that in the former case the stitches must first be cut loose. Rubber belting consists of from three or four plies upwards.
- Mr. Rathjens.** MR. RATHJENS.—Then stitching would have some effect on the friction test?
- Mr. Campbell.** MR. CAMPBELL.—It would be necessary first of all to cut the stitches loose.

MR. C. E. SKINNER.—This same term is generally used in the electrical trade to designate a material that is known as friction tape, which is used very largely for the insulation of electrical apparatus. We are now endeavoring to arrive at some sort of test for the "friction quality" of this tape. We have recently evolved a machine which seems to give a very satisfactory measure of the "friction quality." Later we hope to present this device to the Society in a paper with some of the tests made upon the machine. I agree with the chairman that it is very desirable to get a proper term now to cover this particular test. As used in connection with tape, the test is one to determine the separation of the layers from each other as the tape is unwound from a test roll. The term "separation test" appeals to me as being better in both cases than "friction test." Mr. Skinner.

MR. S. S. VOORHEES.—I should like to ask Mr. Campbell if any work has been done to determine the length of fiber of the cotton used in these conveyor beltings. The Department of Agriculture is attempting to increase the length of fiber of certain of their upland cottons, so as to equal the length of fiber of the sea-island and the Egyptian cotton. The manufacturers, when they are required to furnish high-strength yarn, depend, I believe, more on the amount of twist than on the length of fiber. By this means the yarn may have high strength when spun or twisted, but may lack durability or fail under special conditions of service. Mr. Voorhees.

On material where strength and durability are especially essential, and where the value of the finished product is sufficient to warrant the use of a higher-priced raw material, such as belting to transmit power, automobile tires, etc., it would seem desirable to specify all the characteristics which will insure the required properties. Manufacturers of belting and automobile tires are beginning to appreciate these facts and are investigating the use of a long-staple cotton for their products.

MR. CAMPBELL.—I might say that specifications generally ask for long-staple cotton, but I do not know of any specification which distinctly specifies the length of the staple. It is customary, however, to specify the strength of the duck required and sometimes, too, the construction of the duck is stated—that is, the number of yarns per inch in the warp and filler, and the number Mr. Campbell.

Mr. Campbell. of plies in each yarn. If sea-island or long-staple cotton were used for belting, as it is in the automobile tire business, I am afraid that the price would be prohibitive.

Mr. Voorhees. **MR. VOORHEES.**—It is possible that the price would not be prohibitive, considering the economy of long-staple cotton.

Mr. Campbell. **MR. CAMPBELL.**—That would permit the use of a light-weight fabric, but this would not be a desirable feature under a great many circumstances where weight is necessary as well as strength.

Mr. Waring. **MR. F. M. WARING.**—I should like to ask Mr. Campbell, whether, in looking over the various specifications, he has found that many people were making tests of belt covers. Has he not found that a very difficult thing to do on account of variations in the thickness and quality of the cover, causing great difficulty in getting uniform test specimens, while at the same time experience has shown that it would be very desirable to have specifications for quality of covers?

Mr. Campbell. **MR. CAMPBELL.**—Some specifications set requirements for the cover, specifying the adhesion to the fabric, the tensile strength and ultimate stretch. It should be said, however, that this pertains to belts with thick covers. Belts with thin covers are sometimes subjected to a bending test on the belt itself without the cover cracking under a certain specified pressure.

AN AUTOGRAPHIC FRICTION TESTING MACHINE FOR TESTING MECHANICAL RUBBER GOODS.

By J. M. BIERER.

SUMMARY

On account of the difficulties experienced in determining the friction of mechanical rubber goods, an autographic testing machine has been developed.

This machine eliminates the personal equation of the operator, is simple in operation, and gives a permanent and accurate record of the friction.

AN AUTOGRAPHIC FRICTION TESTING MACHINE FOR TESTING MECHANICAL RUBBER GOODS.

BY J. M. BIERER.

One of the greater problems that confronts every manufacturer of mechanical rubber goods is the standardization of the finished product. Before any appreciable progress can be made it is necessary to have an accurate and reliable method of testing the finished product to detect the variations caused by raw materials and the process of manufacture.

The detailed tests usually employed in comparing mechanical rubber goods are tensile strength, elongation, permanent elongation, and friction. As this paper has to deal primarily with friction tests, the following will be devoted particularly to them.

KINDS OF FRICTION TESTS.

A friction test is the determination of the stress required to separate a unit area of two layers of rubber-impregnated material in a unit of time. In mechanical rubber goods there are three important kinds of goods in which the friction test is indispensable, namely, rubber hose, fire hose, and rubber belts; and in order to appreciate the importance of this test in each of these classes, it is necessary to know their construction.

Rubber Hose.—Rubber hose consists of three parts, an inner rubber tube, a number of layers of rubber-impregnated fabric, and an outside rubber cover. The more firmly these parts are united the greater the stability of the hose, and the greater the service obtained.

In such a piece of hose there are three different kinds of friction: friction between cover and fabric, friction between plies of fabric, and friction between fabric and tube. If the friction between tube and fabric is poor or entirely absent, water in passing through the hose is liable to separate the tube from the fabric and completely stop the flow of water. If the friction is poor between the plies, they are liable to separate in service

and as a result the hose kinks and is useless. By the friction on a piece of rubber hose is meant the stress necessary to unwind 1 in. of fabric, 1 in. in width, in 1 minute. The line of stress is normal to the line of separation.

Fire Hose.—Fire-hose construction is slightly different from that of rubber hose. It consists of a tube of high-grade rubber, a soft rubber or cement backing, and a woven fabric as an outer cover. The object of the backing is to unite the tube firmly to the fabric, and it is essential that this backing shall have sufficient strength to prevent the tube separating from the cover when the hose is in use. By the friction of a piece of fire hose is meant the stress necessary to separate a strip of the tube 1 in. in width from the fabric at the rate of 1 in. per minute. The tests may be made either from a piece cut round the hose or lengthwise of the hose. In this case the line of stress is parallel to the line of separation.

Rubber Belt.—A rubber belt consists of a number of plies of rubber-impregnated duck, with a rubber cover on both sides. Sometimes rubber belts are sewed but usually the sewing is omitted, and the plies of duck are held together by the friction of the rubber-impregnated duck.

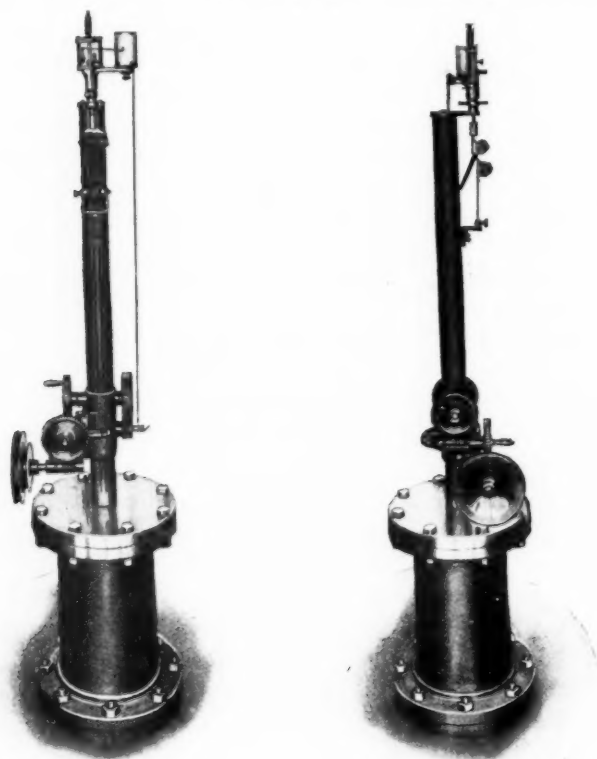
This friction is the only thing that will resist lateral shear, and the instant the plies separate the belt is useless. Other things being equal, the value of the belt is almost directly proportional to its friction. Friction of a belt is determined by cutting a strip 1 in. wide across the belt and determining the stress necessary to separate the plies at the rate of 1 in. per minute. The line of stress is parallel to the line of separation, as in the case of fire hose.

DETERMINATION OF FRICTION.

The actual operation of determining the friction on a piece of rubber hose consists in taking a 1-in. section and mounting it on a freely running mandrel, the last ply of fabric having been unwound for a distance of several inches to receive a grip to which may be attached the necessary weights. It is then necessary to add weights until the separation is approximately 1 in. per minute. This operation would be comparatively simple

if the frictional resistance were constant, but unfortunately there is variation in most rubber goods. It is possible, therefore, to obtain the friction at a few points only.

In determining the friction on fire hose and belts the operation is similar to the method employed in testing rubber hose,



(a) Front View.

(b) Side View.

FIG. 1.—Autographic Friction Testing Machine.

except that they are not mounted on a mandrel, but supported by a stationary grip. Rubber belts and fire hose have a vertical line of separation, while rubber hose has a horizontal line of separation.

In most specifications it is stated that the rate of separation

shall not exceed a certain amount on applying a definite weight. This is a good method of determining whether a lot of hose should be rejected, but it does not give the consumer or the manufacturer any idea of the relative value of different lots of the same kind of hose. For routine factory testing, the specification method is practically useless.

It is easily seen that the present method of determining the friction leaves much to be desired. The personal equation of the operator enters into the test to a large extent, and when there is a decided variation in the friction, it is practically impossible for two persons to obtain the same result, even if they both use extreme care. It is impossible to obtain always the maximum and minimum frictions in a piece of hose, because just when the exact weights have been added to separate the plies at the rate of 1 in. per minute, a weak place in the friction may occur, with the result that before the weight can be caught the hose has unwound and passed over the weak spot and the most essential part of the test has been missed. This method of determining the friction by trial will finally give approximately the correct result.

FRICTION TESTING MACHINE.

What is desired is some method of testing which will give the actual friction for each fraction of an inch for the entire length. In other words, a chart is required on which the variation in friction is plotted against the separation of the plies in the form of a curve.

Recently a friction testing machine was designed which apparently fulfils most of the requirements of a satisfactory instrument. Fig. 1 shows the front and side views, and Fig. 2 a detail view of the top of the machine. It consists of an autographic weighing device mounted upon a suitable frame and connected with a grip which is capable of holding one end of a test piece. The remainder consists of a movable grip connected

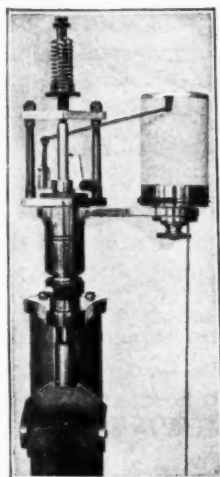


FIG. 2.—Detail View, Autographic Friction Testing Machine.

with a rack which moves in a slot in the main shaft. The rack is moved by a series of gears connected by a belt to a constant-speed motor (not shown). The vertical shaft and base are similar to the same parts employed by the Bureau of Standards on their rubber testing machine.

The autographic part as well as the weighing device consists of a No. 2 Improved Crosby Steam Indicator, with only minor changes. Instead of using a pencil or the usual metallic point on prepared paper, a recording pen is substituted. The paper used may be any coordinate paper, but it has been found that standard profile paper with rulings corresponding to the spring used on the indicator is very satisfactory.

A steam indicator is designed to be operated by fluid pressure. In order to adapt it to a friction testing machine it was necessary to operate it by means of a direct pull, and this was accomplished by connecting a small rod to the lower side of the piston of the indicator, the other end of the rod connecting to one of the grips. In this way any stress on the grip is transmitted through the rod to the piston and tends to compress the spring of the indicator, which in turn moves the pen on the drum.

The principle of the steam indicator is so well known that no explanation of it is necessary. The springs are designed to take either compression or tension, and are all accurately calibrated before they leave the factory; but it is comparatively easy to check them by hanging a definite weight to the upper grip and noting the displacement of the pen. This upper grip is identical with that used on the Bureau of Standards rubber testing machine, and is shown in Fig. 2.

It is necessary to have two interchangeable jaws for the lower or movable grip. The grip required for rubber hose must be capable of holding different sizes of mandrels for mounting the hose, and it is also essential that the mandrel run freely on the supporting pin or axle. Fig. 1 (a) shows this grip. For fire hose and rubber belts the lower grip is identical with the upper grip.

The machine is driven by a constant-speed motor. There is a two-step pulley, giving two speeds. The slower speed lowers the movable grip at the rate of 1 in. per minute and is used for rubber hose. The higher speed separates the grips at the rate

of 2 in. per minute and is used for fire hose and belts. With fire hose and belts the separation of the plies is only one-half that of the grips.

In order to quickly raise the lower grip after a test has been finished, a pin is removed which allows the top worm to disengage the wheel, and it is then easy to raise the grip.

The drum of the indicator is revolved by means of a string which winds up on a small drum, connected with one of the slowly moving wheels.

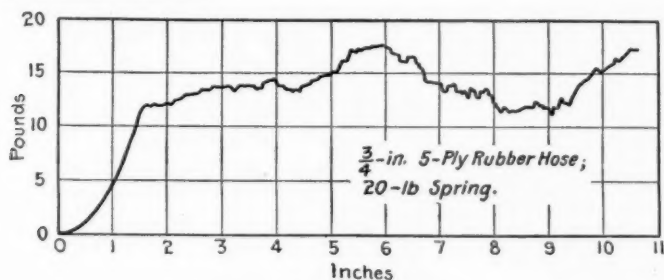


FIG. 3.—Sample Chart.

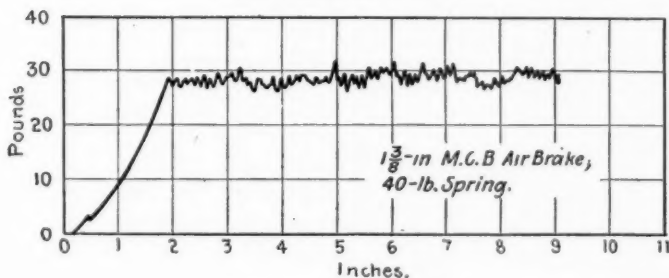


FIG. 4.—Sample Chart.

Figs. 3 and 4 show charts made on the machine. The zero or atmospheric line is drawn by the pen of the indicator in the same way as the atmospheric line is drawn in an engine test.

The accuracy of the machine varies, depending upon the spring being used. With a 20-lb. spring the machine is accurate to $\frac{1}{4}$ lb. With a 4-lb. spring the machine is accurate to 3 oz.

By the use of this machine it is not only possible to make 75 friction tests per day, but a permanent record showing all the variations of the stock is obtained.

DISCUSSION.

The Chairman. THE CHAIRMAN (PRESIDENT ARTHUR N. TALBOT).—This paper is open for discussion. It is evident that there is a field here for developing a standard method.

Mr. Gibbs. MR. A. W. GIBBS.—I should like to ask the author whether the time element, I mean the age, is considered in his diagram. I know that hose a year or so old differs very widely from that which is fresher. If I remember correctly, the manufacturers claimed that there is a certain age at which the best results are obtained, that is to say, the hose would stand a better test when, say, a month old than when new, after which deterioration would begin.

Mr. Bierer. MR. J. M. BIERER.—Age may very materially affect the friction test and certainly for the first few days after rubber is vulcanized, you will get different results than you will in a week or a month. It all depends upon the compounding as to how the material will age. Some frictions may be just as good in a year as they are in a month; they may be just as good in two years, while others may be, in two years, practically useless; that is one of the troubles with rubber goods. There is always a tendency to depreciate with age.

Mr. Edgecomb. MR. H. R. EDGECOMB.—Mr. Bierer in his excellent paper has discussed a friction test for rubber hose, fire hose and rubber belts. These materials have high adhesion between the layers of fabric and the speed of pulling is necessarily slow.

Friction tape, also an important mechanical rubber-goods product, requires a friction test which differs in some respects from the test which the author has described. This tape has a friction coating on both sides and is used by electricians for the protection of bare copper joints, and wherever an insulating fabric is required.

About two years ago we built a rather crude testing machine embodying a pulling mechanism and a graphic recording device. The results obtained by its use enabled us to compare successfully

the friction qualities of various brands of tape. Some time ago **Mr. Edgecomb.** Mr. Bierer showed us the testing machine described in his paper, and as a result of his courtesy we have been able to construct a device which combines the features of his apparatus with

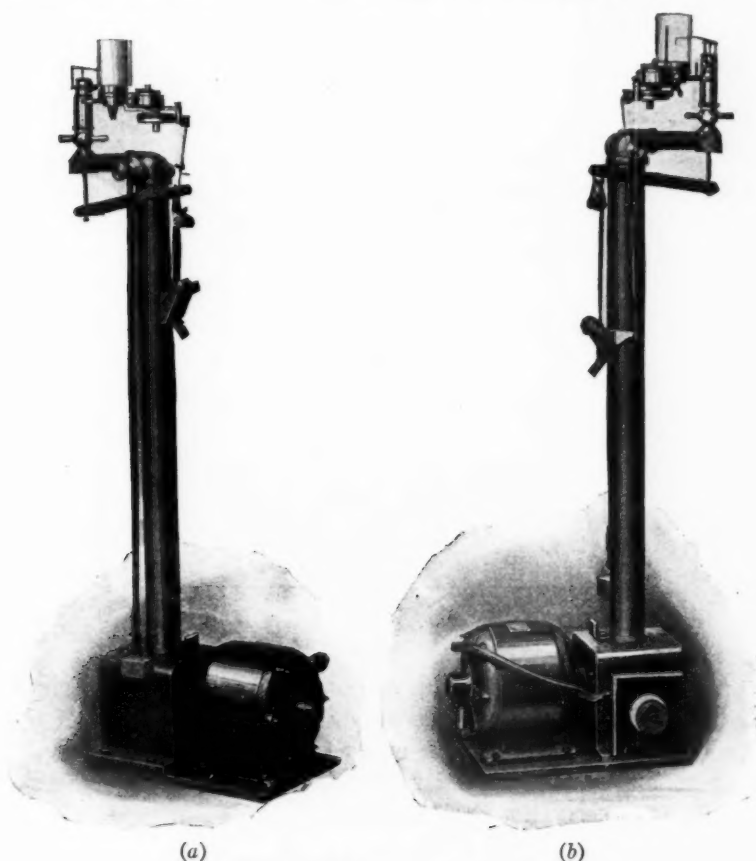


FIG. 1.—Autographic Friction Testing Machine.

certain new features, making it specifically suitable for testing friction tape. Our machine differs from Mr. Bierer's in that it has a device for winding the sample of tape on the bobbin, as will be further explained hereafter, and in that its unwinding or pulling speed is very much higher.

Mr. Edgecomb.

Referring to Fig. 1: A vertical screw within the pillar draws the carriage down at a speed of 30 ft. per minute. The pull resulting from friction between the layers of tape as wound on the bobbin is transmitted to the indicator spring by a beam and thrust pin, which cause a direct lifting of the indicator piston. The registering pencil records the pull on an indicator card the same as in Mr. Bierer's apparatus, except that the zero line is at the bottom of the card instead of at the top. The cord which revolves the indicator barrel is attached to and receives its impulse to move from the vertically moving carriage.

The sample to be tested is unwound from the original roll and wound on the test bobbin, which is mounted for this purpose on the winding shaft, supported in the head of the machine.

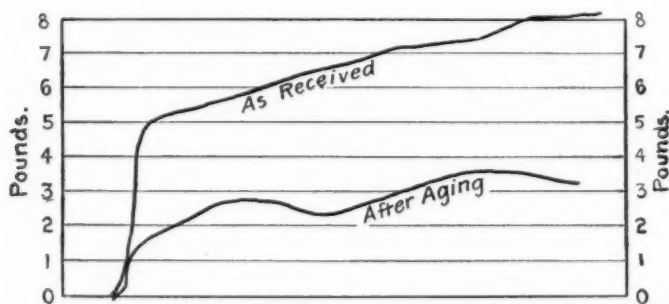


FIG. 2.—Friction Value of Tape before and after Aging.

Rotary motion for winding the sample on the bobbin is obtained by unwinding the cord from a pulley on the opposite end of the winding shaft. This cord is attached to the vertically moving carriage, and when the carriage is raised to the starting position the cord is retrieved on the pulley by a clock spring so placed as to oppose the motion of the shaft, when it is winding the tape on the bobbin. A weight of 5 lb. is attached to the tape sample, so that the tape is wound on the bobbin under a uniform pressure as well as at a uniform speed.

The bobbin of tape is taken from the winding shaft and placed on a shaft in the vertically moving carriage; the free end of the tape is clamped into the clutch which is suspended from the end of the indicator beam. As the carriage moves

down the resistance to unwinding causes a pull, which is registered on the indicator diagram. **Mr. Edgecomb.**

This apparatus provides for rapid testing, as a sample of tape may be wound while the previously wound sample is being pulled.

The friction value of the tape is recorded in pounds as shown in Fig. 2. The tests are made on the samples as received and also after heating 16 hours at 100° C. The latter constitutes an accelerated aging test.

MR. S. S. VOORHEES.—This paper, as well as Mr. Campbell's, **Mr. Voorhees.** seems to indicate clearly the need of definitions. We are using "friction" here for two properties; one is the friction which is obtained in the fabrication of the finished article, and the other is the adhesion which results from winding the material together. If it is within the province of Committee D-11 on Rubber Products, it might be well to ask them to consider this definition as well as some others, which undoubtedly occur and are used, in connection with the products they are considering.

THE SECRETARY.—It may be of interest to state that quite **The Secretary.** recently the officers of the various technical committees have had their attention called to the fact that committees which deem it desirable to standardize terms and definitions in their respective fields, are expected to proceed along these lines with a view of eventually getting up a glossary of technical terms for yearly publication in the Year-Book.

MR. W. K. HATT.—I should like to ask if control is exercised **Mr. Hatt.** over the content of moisture. The control of the moisture in test pieces of woven fabrics is important and the speaker would ask if such control has been provided for in the plan of tests.

MR. BIERER.—In making rubber belts of materials impregnated with rubber, it is usually the custom to dry the duck. **Mr. Bierer.** I don't know that there is any standard of the amount of moisture left in it. After it has once been coated with rubber compound the moisture content will not change very materially.

MR. HATT.—I was speaking of the tests of the fabric itself, **Mr. Hatt.** as brought out in the paper.

MR. BIERER.—The usual custom, I think, in making tests **Mr. Bierer.** on fabric before it is covered with rubber, is to dry it from one to two hours, or even four hours, at 150° F.

Mr. Tilt.

MR. E. B. TILT.—I am much interested in what Mr. Bierer has to say on the subject of friction, and should like to have him tell us what class of rubber products increases in friction during a period up to two years. We have been making a series of tests of air-brake hose covering a period of about six years. I cannot say how many different makers are represented, but certainly half a dozen, including a European maker or two. In no case has there ever been an increase in friction from the time the hose was first tested when new. We have had air hose tested with a 20-lb. weight, and there should be practically no unwinding of the duck. Other pieces would unwind 6 in. during the ten-minute period and again others would unwind completely and immediately with a 20-lb. weight; in fact, a 10-lb. weight would have sufficed, but in no case was there an increase of friction shown in subsequent tests. The Specification Committee of the Master Car Builders' Association is making a series of tests on air-brake hose, including this friction, which will extend over a period of three or four years, and when these are completed we should have some interesting information.

Mr. Bierer.

MR. BIERER.—Very likely the reason you did not get an increase in the friction was due to the fact that the samples had sufficient time to age before your tests were started. The friction of almost all mechanical rubber goods will improve for the first week after vulcanization.

A CRITICAL REVIEW OF THE PROCEDURE GOVERNING THE ADOPTION OF STANDARDS.

BY EDGAR MARBURG.

SUMMARY.

The paper cites, in substance, first, the regulations applicable to the constitution of technical committees, and, second, the by-laws and regulations pertaining to the procedure governing the adoption of standards. The author expresses the opinion that it would be difficult to suggest wherein the former might be materially improved, but in connection with the latter he presents critical arguments as to:

1. The amendment of a committee report by vote at the annual meeting; and
2. The requirement of a letter ballot of the Society for final approval of proposed standards.

Concerning (1) he discusses the present policy by which amendments may be made by a majority vote of those voting, and an alternative policy under which the meeting would not be empowered to amend a committee report, but would have the power only to refer the report to letter ballot of the Society or back to the committee. He concludes that neither of these alternative courses is free from potential evils, and suggests for consideration a compromise policy by which the meeting would be empowered to amend a committee report only by a vote of substantial unanimity,—say a nine-tenths vote of those voting,—whereas a two-thirds vote would suffice to determine whether recommended standards shall be referred without change to letter ballot of the Society or back to the committee for further consideration.

As to the prescribed letter ballot of the Society at large for the final adoption of standards, the author asks whether an experience of 14 years has not shown such a ballot to be almost meaningless, and whether it is not time that it should be abolished.

A CRITICAL REVIEW OF THE PROCEDURE GOVERNING THE ADOPTION OF STANDARDS.

BY EDGAR MARBURG.

The two-fold purpose of the Society, as announced in its charter, is the promotion of knowledge of the materials of engineering, and the standardization of specifications and methods of testing. Without underestimating its notable achievements in the advancement of knowledge concerning materials, it may fairly be held that the Society has found its broadest sphere of practical service in the establishment of standards governing the testing and purchase of materials. The number of committees participating directly or indirectly in this standardization work is now 34 with an aggregate membership of 761. The remarkable performances of some of these committees have been rendered possible only through their creation of numerous sub-committees, of which one committee has now no fewer than 14 and another 12, their findings being of course subject to review by the parent committees. While the Society meets only once a year, its committee activities are carried on unceasingly. Thus a recent inquiry has shown that since the last annual meeting there have been 40 committee meetings and 79 sub-committee meetings, with a total attendance of about 1100. If the average cost of individual attendance at these meetings be conservatively estimated at \$15, this means a total outlay of over \$16,000 in support of committee work for a single year, of which no part is borne by the Society, and in which no account is taken of the time value of the services rendered. The total expenditures out of the general treasury during the past year, fairly chargeable to the committees, is approximately \$1100, which represents substantially the cost of stationery, printing, mimeographing and postage. The fruits of these extensive committee labors appear chiefly in the Year-Book, the last edition containing 64 specifications and methods adopted as standards by the Society, of which many have undergone one or more revisions since their original adoption.

The prestige to which the Society has attained in its own particular field of standardization is attributable, first, to the representative character of its committees; second, their readiness to cooperate at every opportunity with committees of other societies in the broad interest of harmonization, without sacrifice of the right of final independent action; and third, the methods of procedure governing the adoption of standards.

In the early years of the Society's existence, when its membership was small, its committees few, its resources limited, and the groundwork of experience was being slowly laid, it was inevitable that there should have been inadequate restraint upon committee action, and that this condition should have led in turn to criticism—both deserved and undeserved—emanating in large part from its own members, and inspired in the main by a genuine solicitude for its future. Throughout this formative period, with all its perplexities, as well as later years, those entrusted with the management of the Society's affairs have clearly perceived that its opportunity for usefulness was directly dependent on its command of the confidence of those whom it was primarily designed to serve, namely the consumers and producers of materials, or, in a broader sense, the public at large. The prescribed methods in respect to the organization and regulation of committees and the adoption of standards, have been frequently modified as experience directed, with a view of better safeguarding the manifold interests involved, without unduly curtailing freedom of action on the part of the committees, as well as the Society at large. The methods of procedure now in use may be regarded as the gradual outgrowth of suggestions from innumerable sources and the experience of many years. It seems fitting that these methods, which are of such vital moment to the Society, should from time to time be critically reviewed and freely discussed in open meeting with a view of considering whether they are susceptible of further improvement.

It may be well to point out at the outset that the regulations governing the technical committees are formulated and controlled jointly by the Executive Committee and Committee E-5, the latter consisting of the chairmen of all committees dealing with subjects having a commercial bearing, or representatives of their

appointment. These two bodies cooperate under an agreement which need not be described in detail here, although it may properly be stated that in its practical operation, for the past two years, it has proved entirely satisfactory. The regulations in their latest revised form are embodied in the annual report of Committee E-5 presented at this meeting. With respect to the organization of technical committees, these regulations, briefly summarized as to features especially pertinent to the present inquiry, provide in substance:

1. That either an equal numeric balance shall be maintained between producers and non-producers; or that the latter may be allowed to predominate by acquiescence of the former.
2. That the classification into producers and non-producers shall be left to each committee subject to the provisions: (a) that a member who stands in the relation of producer to any product within the province of the committee shall be classed as a producer; (b) that a nominally unattached expert, permanently retained by producing interests in the field of the committee's activities, shall be classed as a producer.
3. That the permanent chairmanship shall be held by a non-producer.

The above regulations are only applicable to committees charged with matters having a commercial bearing, and the classification of their membership into two groups, namely, producers and non-producers, is set forth in the Year-Book and in the annual membership pamphlet. It is a significant fact that on 27 of the 28 committees of this character the non-producers outnumber the producers, and, on the average, in the proportion of seven to four. It seems difficult to suggest wherein the foregoing regulations might be materially improved in a broad consideration of the best interests of the cause.

Let us now summarize similarly the by-laws and the regulations pertaining directly to standardization. The present provisions require:

1. That such matters shall originate in the appropriate committee.

2. That action affecting standards can be taken by a committee only at a meeting called for that purpose; that such action is subject to majority vote of those voting; and subsequently to majority vote of those voting on letter ballot of the entire committee.
3. That the results of each letter ballot, as to the number of affirmative votes, negative votes and members not voting, shall be announced in the report of the committee to the Society.
4. That dissenting members have the right to present minority reports, individually or jointly, concurrently with the presentation of the majority report.
5. That preprints of such reports shall be mailed by the Secretary-Treasurer to the membership of the Society at large not less than four weeks before the annual meeting at which they are to be presented, so that members may be prepared to discuss the subject matter at the meeting verbally or by letter.
6. That amendments may be made at the annual meeting by majority vote of those voting.
7. That final adoption by the Society requires approval by two-thirds of those voting (*a*) at an annual meeting, and (*b*) on subsequent letter ballot of the Society at large.

It would seem that the first five of the above seven requirements are not open to serious question, but that it should be frankly recognized that the last two afford legitimate ground for differences of opinion. Let us present certain considerations, pro and con, affecting these two requirements.

As to the sixth requirement, it may be argued that a vote at an annual meeting should only be allowed to decide whether the recommendations of a committee shall be referred to letter ballot of the Society for adoption, or back to the committee for further consideration. In support of that contention, it may be said, first, that a fairly large and heterogeneous gathering can hardly be regarded as a deliberative body competent to pass judgment on technical details; second, that the right of vote is commanded also by members lacking in expert knowledge of the subject at issue; third, that it is

a physical impossibility, within the limits of time available at annual meetings, to subject the voluminous recommendations relating to standards to critical, detailed review; fourth, that it is unfair to a committee whose report represents the results of assiduous labors on the part of men possessing special qualifications for their task, that that report should be liable to change by a capricious vote, on the part of a body incompetent in a collective sense to exercise discriminatingly the power which it commands, and liable, therefore, to be swayed by specious argument or mere oratory.

Let us now consider, on the other hand, the arguments that may fairly be adduced in support of the regulation under discussion. May it not be held with much reason that an abrogation on the part of the Society of its right to amend committee reports would be unwise on the ground, first, that it would vest too much power in the committees, and that the power remaining to the Society would be purely negative; second, that it would leave no means of promptly making changes, and that even changes concededly meritorious and acceptable to the members of the committee present, would thus have to be delayed for an entire year on purely technical grounds; third, that there would be increasing danger that proposed standards embodying features of questionable merit, on which the committee itself may have mustered only a slender majority vote, would be reluctantly approved on the plea that the only other alternative would mean a year's delay, and that perhaps next year the committee will recommend suitable amendments, whereas the Society, although the creative body, would then command no power to compel such action.

Enough has been said, it is believed, to show that the question under discussion is a perplexing one, and that neither of the alternative courses presented is free from potential evils. A compromise policy seems worthy of consideration by which the Society in annual meeting assembled would be empowered to change the recommendations of a committee only by a vote of substantial unanimity,—say a nine-tenths vote of those voting,—whereas a two-thirds vote would suffice to determine whether such recommendations shall be referred without change to letter ballot of the Society, or back to the committee for further consideration.

Another policy, by which proposed changes in the recommendations of a committee would be referred by vote to the Executive Committee with discretionary power, would merely serve to shift upon a few individuals the responsibility of passing final judgment upon technical questions with which collectively they are rarely qualified to deal. Such an extension of the powers of the Executive Committee would seem, therefore, to be inappropriate and injudicious.

Turning, now, to the last of the above seven requirements governing the adoption of standards, the value of a letter ballot of the Society as the final step in this procedure may well be brought into question. To clear the ground for discussion, should it not be conceded that in a practical sense, and as attested by an experience of 14 years, the letter ballot is almost meaningless? In all these years every proposed standard that has survived the cross-fire at annual meetings has been approved on letter ballot by a substantially unanimous vote, but one representing after all only a small fractional part of the membership. Thus the average total vote on the 32 proposed standards submitted to letter ballot a year ago was only 95 from among a membership of 1620, the average affirmative and negative vote being 92 and 3, respectively. On the other hand it may be held that standards cannot, strictly speaking, be said to have been adopted by a Society unless every member, whether present at a given meeting or not, has been given an opportunity to vote by letter ballot. But should this consideration alone warrant the continuation of a performance which long experience has shown to possess little or no practical significance? Is it not true that an endorsement, after debate, by at least two-thirds vote of a fairly large and representative part of the membership, is a sufficiently exacting final test, and if not, that a subsequent letter ballot will not serve to remedy matters?

It has been suggested that proposed standards when submitted to letter ballot should be accompanied by a full report of the discussion thereon at the meeting at which they were introduced, in order that those who were not present may be enabled to vote intelligently. But, apart from the expense and delay involved in such a course, and the difficulty of editing *ex parte* arguments, especially designed to influence votes, is

it not true that members really competent to vote need no such aid, and that those not competent to vote are apt to be bewildered rather than helped by expert testimony of equal positiveness, pro and con, on subjects with which they are comparatively unfamiliar? Moreover, may it not well be doubted that busy men will read and weigh lengthy discussions on subjects in which they have no immediate interest, in a conscientious endeavor to cast their votes as intelligently as possible? If this plan were sanctioned, would not the yearly contentions of opposing factions in making their respective appeals to "voters," tend to keep the Society perpetually in hot water, to its great detriment? In short, may this proposal not be regarded as an added argument for the abolishment of the letter ballot in the adoption of standards? It seems pertinent, in that connection, to consider that so representative a body as the American Railway Engineering Association has never included the requirement of a letter ballot in its procedure governing the adoption of standards, although certain discretionary judgment is vested in the Board, by which it may set aside the action at a convention.

In a study of the general problem before us, it seems well to recognize that the responsibility as to standards must rest primarily with the committees. That condition may be regarded as unalterable as the law of nature that a stream cannot rise above its source. Are we not blinding ourselves to this condition, and disguising it by an unduly elaborate course of procedure in the adoption of standards emanating from our committees, and would it not be better for us to face it frankly, and to take such measures, if any, that may be needed to place our committees above reasonable criticism as to their personnel and methods? The procedure of the committees, as prescribed now by the general regulations applicable to all committees, and by the special supplemental regulations which some of the larger committees have framed to meet their individual needs, leaves apparently little to be desired. But wholly apart from any controlling regulations, it has come to be recognized in increasing measure that no provision in a proposed standard specification can wisely be regarded merely as a subject of majority vote

in the committee concerned, but that it should receive practically the unanimous endorsement of the committee to be of value. The consumers on a committee realize that their cooperation on specifications would be a waste of time if their efforts to get standards that they would be willing to use in their own practice should be unfairly blocked by the producers; or if, on the other hand, they, themselves, should force unreasonable requirements into specifications which would merely serve to make them a dead letter in practice. It is in this spirit of reasonable cooperation between both parties in interest that standard specifications are now being evolved by our leading committees, and it is for reasons just stated that the producers are not disposed to object to being so largely outnumbered on certain committees by the consumers. Thus the records of Committee A-1, on Standard Specifications for Steel, show that the total attendance at the seven meetings held since January, 1913, was 251, divided in the proportion of 158 to 93 as to consumers and producers respectively.

An important added safeguard in connection with committee reports, which has become effective for the first time at this meeting, is to be found in the provision that every report must contain an analysis of the committee ballot on every proposed measure affecting standards, as well as on the report as a whole. Thus it becomes apparent at a glance whether a given recommendation is presented to the Society with the practically unanimous support of the committee, or in consequence of a slender majority vote, so that it may be received with confidence or with reserve, as the case may be, and action may be determined accordingly.

In order that a committee may realize its full possibilities it is clearly its duty to see that its personnel is representative of every important interest in its field; that meetings are held with sufficient frequency and under conditions calculated to encourage active participation on the part of its members; that drones and figure heads are eliminated and replaced by live workers; that its scope of activities is enlarged as opportunity offers; that cooperation with suitable committees of other organizations is encouraged; that recommendations from every proper source shall receive prompt and impartial consideration—in short, that it shall be keenly alive to its obligations to itself

and to the Society, in all matters coming within its legitimate province.

In conclusion it may be said that this review has been prepared for the purpose, already stated, of eliciting helpful criticism through free discussion in open meeting and that it represents merely an expression of the author's personal views. It may also be stated that the question of abolishing the letter ballot for the adoption of standards has never been made the subject of special consideration either by the Executive Committee or Committee E-5.

DISCUSSION.

THE CHAIRMAN (MR. A. W. GIBBS).—Gentlemen, the Executive Committee would very much appreciate suggestions from any member who may wish to offer them. The subject has come up at our meetings several times. One proposal was that the members should be advised that if they were not posted on certain subjects submitted to letter ballot, or were not interested in them, they should refrain from voting. The question is a very serious one and any member who has any views on the subject will be very welcome to express them. The Chairman.

MR. G. L. FOWLER.—I have been going about among the railroads and have found a great many men who have even spoken disrespectfully of the specifications of this Society on the very ground Mr. Marburg has brought up, and that is that the specifications are passed by a body of men who do not know anything about what they are voting on. What do I know, for example, about the specifications for lime? I presume I could tell lime from poor wheat flour, but that is about all, and there are many other members of the Society who are specialists along certain lines, but who are absolutely incompetent to vote on specifications along other lines. Yet, when a matter comes before the Society, it is hardly fair that an absent member who is an expert should be denied the privilege of voting. It may sound something like a joke, but it seems to me that the proper way of sending out these specifications for letter ballot would be to precede them with a notice to the effect that, "If you know anything about this subject, vote and vote carefully; if you don't know anything about it, don't say anything." That would shut out men who are voting conscientiously but ignorantly, and the result would be, possibly, that we would get a vote of men who are experts along the line of the specifications voted on, and silence on the part of men who know nothing whatever in regard to the subject under consideration. Mr. Fowler.

MR. W. P. BLAIR.—For every member of the Society to vote on questions which arise in which he is not interested or Mr. Blair.

Mr. Blair. of which he knows nothing, I should not think either desirable or wished for by anyone. Speaking personally, I should not want to vote on nine-tenths of the questions that come before this Society because I am not competent to do so.

The Secretary. THE SECRETARY.—I should like to be able to share Mr. Fowler's hopeful feeling that a notice in connection with a ballot would serve the very useful purpose he indicates, but from long experience in calling attention in bold-faced type to things that one would imagine no man need have his attention drawn to, and realizing, as I do, how frequently, through some strange perversity of human nature, such things are overlooked, I cannot share Mr. Fowler's optimistic feeling. Such directions would accomplish the desired purpose if every man would scan them and heed them, but abundant experience shows that they won't.

Mr. Wig. MR. R. J. WIG.—If the Secretary would refer back to several committee reports that were presented in previous years, he would find some of them could not have been sent back to the committees if the rule he suggests, requiring a two-thirds vote, were adopted. Those present at an open meeting of the Society usually vote to sustain the committee especially if they have no particular knowledge of the subject under discussion. It was under such circumstances that the reports I refer to were referred back to the committees by a bare majority vote. At this time, however, committee reports were often written by one individual which is not possible under the present rules.

Mr. Miller. MR. R. P. MILLER.—One thought has been suggested by one of the speakers, that while the letter ballot, as shown in Mr. Marburg's paper, seems to be of little value as expressing the real sentiment of the Society, the members of the Society ought, at least, to have some notice of the proposed adoption of any standard. Not all the members who are interested and competent to pass on a certain subject are on the committee, and those who are competent ought to be informed and given an opportunity to express their views on the adoption or rejection of a proposed standard. While a letter ballot does not seem the desirable thing, some sort of a general notice to the Society ought to be observed.

Mr. Skinner. MR. C. E. SKINNER.—As a member of the Standards Committee of the American Institute of Electrical Engineers, I have

had frequent occasion in the last two years to hold up to the standards committee of that society the work of this Society as a standard. In like manner, I might at this time call attention to the method which they use in the adoption of standards. Their committees report "Recommended Standards" to the Board of Directors, and those become standards or not, depending on the action of the Board. Such a plan has been mentioned by Mr. Marburg and others, but Mr. Marburg does not seem to consider the plan practical. It seems to me, however, that if it were definitely known that the standards were to be adopted either by vote at the Annual Meeting or by vote of the Executive Committee, all arguments for and against would be presented in time for the consideration of the body having jurisdiction.

THE SECRETARY.—I do not like to appear in the attitude of one seeking advice and then talking against such advice as is offered. Yet, in my judgment, it would be very inadvisable to have the responsibility placed upon the Executive Committee, as has been suggested by Mr. Skinner, of casting a final vote on the adoption of standards. These standards are of so varied a character, and the membership of the Executive Committee, like that of the Society, consists of specialists in so many different lines, that as a body they would usually be incompetent to sit in expert judgment on proposed standards. In fact, in some instances not a single member of the Executive Committee might be qualified for such a task. In other instances, a single individual might possess the proper qualifications, in which case the action of the Executive Committee would practically have to be governed by the recommendations of that individual, or his recommendations would have to be disregarded by men not technically competent to form independent judgments on the subject at issue. If the matter is viewed in this practical light, it seems to me we can hardly fail to recognize the weight of these objections.

MR. SKINNER.—As I understand the situation, I believe a favorable vote of the Executive Committee would be a vote of confidence in the technical committees rather than a vote as experts on the subject.

THE SECRETARY.—Would that not practically make a rubber stamp of the Executive Committee?

Mr. Miller.

MR. MILLER.—In further explanation I understand that any proposed specification or standard would come before the respective committee for its approval. When such approval and the confirmation by the Society at its annual meeting have been given, the general membership should be informed of that fact so that another opportunity may be had to enter protests. With regard to the point that the members do not look at these things and pass them over, I do not think that is as general as perhaps it is supposed to be. I think it will be found that most of the members do examine those papers and reports in which they are interested. I have in mind what happened two years ago, when certain specifications came up dealing with re-rolled rails. There was quite a protest by those who were affected by the proposed specifications. When members have something to say, they say it; otherwise it may be assumed that they approve.

Mr. Hess.

MR. HENRY HESS.—I want to say a word for the Secretary. Practically every suggestion made by the last half-dozen speakers is in effect today. I am a member of a number of other societies and know that the same difficulty is encountered in these other societies that is met with here. The members do not pay attention to matter sent them, under whatever guise, and you cannot compel the attention of more than the exceptional few.

In regard to the adoption of standards: it is the experience of the entire engineering world, that no society can make a standard; all that a society can do is to canvass a subject thoroughly and suggest standards. It remains for the engineering public to put the suggestion into practice and by such adoption, actually to make the standard. I would therefore suggest the practice that is in vogue in some other societies, that a proposed standard be first recommended by the Society as "recommended practice," and that it receive the seal of the Society as a "recommended standard" only after no opposition has developed during a certain length of time, which time will have to be set by those who know most about the subject, and which for some things may be a year and for others may be five years. If by that time the standard has been adopted by the public through its going into use, it may be called "standard" with no qualifying term. The wisdom of this suggestion will be recognized when it is

recalled that even so important a machine element as a screw **Mr. Hess.** is not really standard; dealers will supply as "United States Standard" screws varying in heads, even in pitch, to an extent precluding interchangeability. If such a thing is possible after so long a lapse of time with so fundamental a thing, the absolute futility can be realized of any society after a mere letter ballot and a short time calling a thing a standard.

I should also like to refer to a broader question. This Society has set itself as one of its chief tasks, the consideration of things that go to make standards; practically every engineering society in the United States and in the world is doing the same thing. Every committee in this Society, although not to the same extent as in others that I know of, makes its own laws as to its fundamental procedure. There is no necessity for that, no reason in the world why all committees of this or other societies should not have a common basic practice, just as the courts all over the world have certain basic practices. The American Society of Mechanical Engineers, one of our oldest ones, much older than we are, has done standardizing work almost from its inception. I have the honor to be a member of the council of that society and to be chairman of a committee of standardization in general. That committee does not concern itself with any individual standard, but has for its object simply the unification of the basic practice of the various committees. That committee has expanded its work to secure cooperation with the other engineering societies, not only in the United States, but also abroad, and has already secured considerable adherents in Europe and at home. I would present to our membership as well as to our governing body, the desirability of such cooperation. I have studied with much interest our own committee practices, have compared them with what there is in existence of formulated practice elsewhere, and I think I can say that, with a single possible exception, our formulated basic practice is the most thorough. Our Society is, therefore, peculiarly well fitted to enter into cooperation.

MR. ROBERT W. HUNT.—I think there is no technical **Mr. Hunt.** society in the world, that has been as fortunate as ours in the loyal work of its committees. In fact, the life of this Society is in the work of its committees. They have been carefully

Mr. Hunt.

selected, and the results which we have had from them during the years of our existence have certainly justified the selections. They have given us their best efforts. It is as a monitor, perhaps I may so call it, that the Executive Committee has its controlling hand on their work. Their work which comes to the annual meetings, as has been stated by the Secretary, is given to the whole membership in all of its detail, in ample time for every member of the Society to be fully cognizant of what they recommend for adoption. Now, a number of other societies adopt nothing except for recommendation, as, for instance, the American Society of Civil Engineers, which I believe is the oldest technical society in the United States; they do not adopt standards; they accept the reports of their committees recommending certain specifications. Our Society adopts certain specifications; they become ours, and they are known as ours without any strings to them at all.

Now, we have another reputation, and a most enviable one, that is, of all the technical societies of the United States; we have the reputation of doing perhaps the most earnest work at our meetings. There is more attention given to work and less to play than by practically any other national organization. As stated, we come here, notified in advance of the work which is recommended for adoption by our committees. The parties who are in interest have every opportunity to come here and discuss that work and vote on it, and I think it ought to be settled here finally. The subsequent ballot by the general membership isn't worth *that*. In the first place, you don't get a general expression from our membership, and you have the danger—a danger which has never happened, but still a danger—of an overwhelming negative vote from people who are uninformed on the matter. Here is the place for the vote; if it is worth voting on at all, let us vote here and let that be final, so that when we adjourn, we have either approved of the work of our committees or we have said to them "Gentlemen, take time and give further consideration to the subject."

The Chairman.

THE CHAIRMAN.—Mr. Hunt, will you not amplify your remarks by stating what kind of a vote you would suggest? Mr. Marburg has recommended a very large vote.

MR. HUNT.—The question is whether it shall be a two-thirds vote; I think that is Mr. Marburg's suggestion, isn't it? Mr. Hunt.

THE SECRETARY.—No, sir, I suggested that it might be worth considering whether a nine-tenths vote should not be required to change a committee report; a two-thirds vote being required for reference to letter ballot of the Society or for recommittal. The Secretary.

MR. HUNT.—That is another question worthy of discussion. My point is, however, that such things should be settled finally at our meetings and that we should do away with the farcical letter ballot. Mr. Hunt.

PRESIDENT ARTHUR N. TALBOT.—I think we are all agreed that the work of the committees constitutes the greatest strength of the Society. I am not one, however, who believes that all the ability of the Society or all the knowledge of the Society rests with the membership of particular committees. Probably in Committee A-1, 110 in number, they have a monopoly of a knowledge of iron and steel. Probably in some other committees this state of affairs may not exist. I wonder, though, whether these committees would want not to be criticised. Is there not in the possibility of criticism, in the attempt to amend, a spur to committees to bring in a report which will not be criticisable? I do not refer now to the more active, energetic, efficient committees, but to some other committees that the Society has unfortunately had in the past and possibly may have in the future. If a committee report is not subject to amendment, and that seems to be the purport of some of the remarks here, then let us say that the Society is made up of a number of independent committees or sections, and that the Society at large is not responsible for the work of these committees. I believe that the change in by-laws which has been passed to letter ballot, requiring a two-thirds vote before amending the report of a committee, is a desirable one. Possibly it should be three-quarters, and yet I think there is no harm done, no danger in leaving this at the two-thirds vote. The President

Now, as to the adoption of specifications by passing them to letter ballot, which has been called farcical; it has been said that there were only 90 votes cast last year on an average, or something like that. I have seen adoptions of reports made at meetings of this Society when there were not 15 votes cast and

The President. very little interest, apparently, among those who voted for it. Is there not an advantage in the letter ballot in helping to give confidence to the action of the Society? Does not the use of the letter ballot give a character to the adoption of reports and specifications that will not be obtained without it, when they would not be accepted so fully by the outside public, if it became known that a handful of people here have voted for the adoption and that the matter was not submitted to the membership at large? It has been said, of course, that these proposed amendments and reports have been submitted to the membership at large for a time before this meeting, and that therefore they have had ample notice. There are busy men in this organization; there are men who are interested in various phases of the work, and it is not to be expected that they will have the full opportunity to go over this and make their reports within that time. It is barely possible, too, that some action taken by the meeting here may not be acceptable to the membership and there is chance for a check in submitting it to letter ballot of the Society.

Reference has been made to the fact that the American Railway Engineering Association the adoption of standards is made by the meeting, in open meeting. It ought to be said, also, that in that case there are present perhaps 15 or 20 of the committee, and that there are 300 people voting, each one of whom feels that he is about as competent to pass on that subject as the men who make the recommendation, and that that meeting does not hesitate to amend in any particular any part of the report which it sees fit to change, and that on a bare majority vote.

It seems to me that there are enough advantages in the letter ballot for the Society to retain it; in fact, it has been my hope for some time that the Executive Committee would see fit to send out the blanks for letter ballot by letter mail, rather than putting them in with a lot of other third-class material which may not come to the attention of the recipient at once. If some care were taken to emphasize the letter ballot rather than to consider it merely a farcical action, I believe that the result in that respect would be better, and I feel sure that the retention of the letter ballot will be of great advantage to the Society.

Mr. Thackray. **MR. G. E. THACKRAY.**—In this connection I wish again to express my appreciation of the able manner in which our Secre-

tary has presented his valuable ideas, and called attention to the defects in our letter-ballot system. It seems that the present method is democracy run riot, namely, to leave to 1700 people the determination of questions in which perhaps only a hundred may be interested or may be conversant with. It does not seem to be the fair thing. The natural and logical solution of the question appears to be the division of the Society into a number of sections, each one of these composed of men conversant with the subject with which they have to deal, those sections again appointing a large proportion of their number as a committee to act upon the question before it. Whether the committee should report to its section is not a matter I am prepared to pass upon at the present time, but it seems to be illogical and unreasonable to submit to letter ballot of the whole Society a subject regarding which they know less in general than a properly constituted committee or section. For several years past the Secretary has not had a general vote from me on a pink slip, for this reason: I have looked it over and have been absolutely overwhelmed to see how little I knew of its various subjects, so I threw the ballot in the waste basket, and from the figures presented by the Secretary I see that most of you have done the same.

MR. CLOYD M. CHAPMAN.—I have listened with a great deal of interest to the many proposals that have been made, and out of it all stands most prominently the suggestion just made, namely, the possibility of subdividing the Society into sections. It has been forcibly brought out that the organization is not, as a whole, competent to pass on a large number of questions that come before us. Very few, if any, of the members are sufficiently conversant with the greater proportion of our activities to vote intelligently, or care to vote on many of our questions. Our committees are made up of specialists in their particular line. We have not, on those committees, all the members who are specialists in the particular line covered by the committee. We have also many men who are not specialists in a particular line, but who are sufficiently interested and know enough about that line to want to have a right to express an opinion. Therefore, voting should be done by more members than those who compose the particular committees interested. There are members outside of the com- Mr. Chapman.

Mr. Chapman. mittees who may know as much about the subject as members on the committee, and there are others who perhaps know less of the subject and yet know enough to know their own needs and want to express an opinion, and are willing and ready to vote, and who will vote with sufficient intelligence to lend authority to their vote. To accomplish this would it not be possible to subdivide the association into groups by allowing each member to register in as many groups as he is interested in, and allowing him a vote on the passage of any standard specification or the reports of committees in the various sections in which he is registered? We would then not submit lime specifications, for instance, to the whole 1700 members, but submit it to the 16 of the committee and to perhaps 100 more who are sufficiently interested to want to vote on lime specifications and who have registered in the lime section. Some of our members are connected with general research laboratories and may be interested in every one of our divisions or in half of them or half a dozen of them. These would be registered in all the divisions in which they possess sufficient knowledge or interest. The process of adopting proposed standard specifications would be the same as it is now up to the time of the meeting. They would be printed and distributed; everyone would have them, and anyone who had not registered in a section in which he was interested could register then, and the meeting could discuss and refer to letter ballot for adoption, or amend, or refer back to the committee. Then, instead of submitting all the specifications to letter ballot of 1700 members, each specification would be submitted to as many men as are registered in that section in which it belongs. Some of the specifications would then be voted on by perhaps half of the membership or a quarter of the membership thus sectionalized, subdivided into groups composed of the men, who take an interest in that particular subject and register in that particular section.

Mr. Hughes.

MR. E. E. HUGHES.—It seems to me that we are straining at a gnat. In thinking over the procedure of the Society, I don't see how we can improve it very much. As I understand it, we start with a sub-committee, let that be the jury; all facts in regard to a proposed specification can be presented to that sub-committee, and that sub-committee can take all the evidence

and testimony that is possible. That sub-committee reports to **Mr. Hughes.** the main committee, the main committee can then take up the subject further if it sees fit, and the committee then presents its report to the Society. Now, by that process, it seems to me, we have had a thorough sifting out, and have got a good general result. When the report comes before this Society, we have another opportunity. I think that the good sense of the Society, as represented by the members who attend these meetings, will realize that the sub-committee and the main committee have had the proper opportunity of going into the subject very thoroughly, and they are going to look with a great deal of respect on the results obtained by these committees. Now the Society may pass what the committees have found to letter ballot. Why not give every member of the Society who has an equal right to a vote on every subject, an opportunity, if he sees fit to exercise it, to vote on a subject? If he sees fit to throw the ballot into the waste basket and not act upon it, that is his own fault, but every one of the 1700 members can say when something has been standardized: "If I was opposed to that particular thing, I had the opportunity to vote against it; if I was for it, I voted in favor of it." I don't see how we are going to better things by doing away with the letter ballot. By doing that we take every member's kick away from him, because he has been given an opportunity, as a last resort, to vote on every subject.

MR. RICHARD MOLDENKE.—I should like to move that this **Mr. Moldenke.** matter be referred to the Executive Committee for action.

THE CHAIRMAN.—I think it is in the hands of the Executive **The Chairman.** Committee now. I shall ask the Secretary to close the discussion.

THE SECRETARY.—I should like to call attention, first, to **The Secretary.** the fact that the regulations governing the adoption of standards, in so far as they are not prescribed in the by-laws, are entrusted to two committees of which one is the Executive Committee and the other Committee E-5 on Regulations Governing the Form but not the Substance of Specifications, which consists of the chairmen of the committees concerned with standards. These two committees exercise a mutual check upon each other and thus far have operated in entire harmony. The present discussion is, therefore, really informal and not

The Secretary. designed to crystallize into immediate action. As expressive of the sentiments of various members it will serve a useful purpose, however, in the further consideration of this matter by the previously mentioned committees.

It is, of course, always easier to point out difficulties and to offer destructive criticism than constructive criticism. Again, constructive criticism may look promising at first sight and appear in a different light when its practical application is considered. Take, for instance, the sectional divisions that have been proposed by Mr. Thackray and Mr. Chapman, and consider for a moment how difficult it would be to make the proposed classification in view of the highly specialized and varied character of the standards with which the numerous technical committees have to deal. Under the present system every member can automatically classify himself as occasion arises, in connection with letter ballots on proposed standards, by voting or refraining from voting on matters thus submitted, without making it incumbent upon him first to notify the Secretary as to the subjects on which he feels competent to vote, and to which afterwards his right to vote is to be restricted. The number of sections that would have to be created if such a scheme were to be put into effect, and the amount of correspondence and bookkeeping that would be entailed in maintaining the record of the sections with which each member might wish to be identified, would involve an amount of clerical work, with apparently little or no gain, which can hardly be overestimated.

As I see it, the principal questions are, first, whether the general letter ballot in its present form is to be adhered to or abrogated, and second, whether the annual meeting shall be empowered to amend committee reports by a majority vote, as now prescribed in the by-laws, a two-thirds vote as prescribed in the proposed revised by-laws, or by a larger vote approximating more nearly to one of substantial unanimity.

(Addendum, by letter.)—Upon reviewing the discussion it seems desirable to enter briefly upon a few of the points raised by various speakers. For convenience of reference these will be taken up in the sequence of the remarks to which they apply.

It is difficult to understand why the standard specifica-

tions of the Society should be held in disrespect in any quarter on the ground alleged by Mr. Fowler, namely, that as a final act in their adoption these standards were referred to letter ballot of the Society at large. The wisdom of adhering to this policy may well be questioned, as has, indeed, been done in the paper, although with no thought that its continuance or discontinuance might serve to raise or lower the high estimate which now attaches to the standards promulgated by the Society. It would seem evident, however, that a particular feature in the procedure of adoption can not affect the intrinsic merits of the standards themselves, and this is clearly the only ground on which they may fairly be judged. The Secretary.

In reply to Mr. Miller's remarks, it may be said that the paper contains no suggestion towards abrogating the present practice by which committee reports embodying recommendations affecting standards must be mailed to the membership at large not less than four weeks before the annual meeting at which they are to be presented, and by which every member is given an opportunity to discuss such matters at the meeting verbally or by letter.

Mr. Hess suggests that a proposed standard shall first be designated "recommended practice;" that eventually, "if no opposition has developed during a certain length of time," its designation shall be changed to "recommended standard;" and that finally "after it has been adopted by the public through its going into use" it shall be called "standard," with no qualifying term. It would seem that the adoption of these three terms would not only be confusing as to their relative meaning, but would lead to serious difficulty in determining when one designation shall be superseded by the next. It should be borne in mind, in that connection, that every standard of the Society is in effect a "recommended standard" or a "standard *pro tem.*," subject to change from year to year in the light of increased knowledge and experience. If the standards are what they should be, the standards for any given year are in accord with representative current practice, and nothing more should be claimed for them. The adoption of the term "standard" in the sense proposed by Mr. Hess would, on the other hand, be suggestive of a degree of fixity and finality incompatible with

The Secretary. the implied meaning of that term under the policy wisely adopted by the Society from the first, by which its standards are all subject to amendment. Under the classification suggested by Mr. Hess, a "standard" would either cease to be subject to change, or, if changed, would again have to go through the evolutionary process, as to designation, from "recommended practice" to "recommended standard," and finally to "standard." It may be said further that the adoption of a graded series of terms in the designation of standards would tend to impair confidence in proposed standards under the headings "recommended practice" and "recommended standards," in comparison with "standards;" and finally, that the acceptance and use of our standards—in the present well-understood meaning of that term—is not made obligatory upon any one.

In reply to Mr. Talbot, it may be pointed out that there is nothing in the paper, nor in the discussion, that would appear to warrant the inference that any one assumes that "all the ability of the Society or all the knowledge of the Society" concerning a given subject is centered in a particular committee. Committee A-1, too, would doubtless disclaim the "monopoly of a knowledge of iron and steel" attributed to it. Proper emphasis has, however, been laid on the consideration that the committees always should be, and usually are, composed of a sufficiently large number of individuals who possess special qualifications for the task committed to their care, and that the findings of properly constituted committees should not be lightly subject to amendment at annual meetings "by a capricious vote and on the part of a body incompetent in a collective sense to exercise discriminatingly the power which it commands." Nothing, it may be added, is more discouraging to a committee which has discharged its duty to the best of its ability than the misuse of such power. If a committee is not properly constituted, its personnel should be changed or enlarged and, if need be, at the direction of the Executive Committee, but that condition should not be permitted to react detrimentally upon other committees whose make-up is above criticism. While no one has advocated, and it is hardly to be assumed that any one would advocate that committee reports shall not be subject to criti-

cism and amendment, as Mr. Talbot appears to have inferred, The Secretary. it is important to distinguish between the power "to amend" and the power "to refer back." No one has suggested a vote higher than a two-thirds vote for the purpose of "referring back." The possibility of "amending" a committee report in ways unacceptable to the committee by a like vote is, however, a wholly different matter and one fraught with dangerous possibilities, which, if they should be realized, would almost inevitably lead to serious protests from the committees affected. Whether such experiences will actually be had can, of course, not be foreseen, although there have already been near approaches to such experiences. In the meantime it would probably be best, all things considered, to adhere to the recently adopted plan by which committee reports may be amended only by a two-thirds vote, as against the previously governing majority vote.

As to the adherence to the letter ballot as the final step in the adoption of standards, the ballot taken since the annual meeting, although manifestly cast much more discriminatingly than heretofore, shows no departure from previous experience in the smallness of the negative vote, this vote ranging from 0 to 7 in a ballot on 31 items, in which 355 members participated. As attested by this recent experience as well as a previous experience of 14 years, the letter ballot would therefore appear to be of little or no practical value. Moreover, since the Society has frequently been criticized for adhering to that form of final vote on standards, the question of its continuation or abrogation is plainly one deserving of serious consideration.

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